It's Not Easy Being Green:
Lessons from Disposable Carryout Bag Regulations

by

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A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Agricultural and Resource Economics

in the

Graduate Division

of the

University of California, Berkeley

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Spring 2017

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Abstract

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What and how we eat impact our environment, and our environment impacts how and what we eat. With public concern over reducing pollution and health care costs, policymakers often turn to economic interventions to change how consumers consume (e.g., taxes, bans, and advisory campaigns). In this dissertation, I explore the effectiveness of such policies, especially when there is debate over optimal policy design. I also examine how these interventions displace consumption in unintended ways—where the reduction of one externality causes the growth of another. Finally, I consider how these policies interact with issues of equity. Environmental and health issues often disproportionately affect poorer populations, and yet sometimes the policies designed to address environmental and health issues have unattractive distributional consequences.

In this dissertation, I focus on and dissect one increasingly popular policy intervention which aims to address an environmental externality by incentivizing consumers to change how they obtain food—*the regulation of disposable carryout bags (DCB)*. The contentious debate over these policies and their design, as well as the spatial and temporal variation in their adoption across the U.S., make them a particularly rich setting to study. The three essays of my dissertation examine DCB regulations in three distinct ways: (1) are they effective tools for changing behavior, (2) do they impose non-monetary costs on consumers, and (3) do they cause unintended consequences that undermine the benefits of the policies.

The first chapter in my dissertation, ***Bans vs. Fees: Disposable Carryout Bag Policies and Bag Usage***, examines the importance of policy design. Having lived in both Washington, DC—which implemented a plastic and paper bag fee in 2010—and in Berkeley, CA—which implemented a plastic bag ban and paper bag fee in 2013—I wanted to know whether bag bans or bag fees were more effective in changing behavior. Bag bans are command-and-control approaches to regulate behavior directly while bag fees are market-based approaches to incentivize individuals to change their own behavior. While there is growing adoption of both types of policies in the U.S., little had been done to compare outcomes under each regulation tool. To fill this gap, I designed a field experiment taking

advantage a local DCB policy change in the San Francisco Bay Area. With help from a team of undergraduate researchers, I made bi-weekly visits to a set of treated and control stores during the months before and after the DCB policy change. We observed customers during checkout and recorded the number and types of bags used, whether a bagger was present, and basic customer demographic information. With these data, we use a difference-in-differences model to measure how bag bans affect customers' demand for various types of disposable and reusable bags. We then investigate how bans and fees compare by juxtaposing our analysis with a concurrent study on bag fees in the DC Metropolitan Area. We find that both policies lead to remarkably similar increases in reusable bag usage and decreases in total disposable bag usage. However, under a plastic bag ban, the eradication of plastic carryout bags is offset by a 47 percentage point increase in the use of paper carryout bags. Therefore, if the environmental costs of *both* plastic and paper bags are a concern to policymakers and the public, our results indicate that the policy tool matters.

The second chapter, ***Waiting in Line: The Hidden Time Costs of Changing Behavior***, asks whether DCB policies impose non-monetary costs on consumers. Understanding the non-monetary costs consumers face has implications for social welfare evaluation and policy design; however, quantifying these costs is not always feasible. In this chapter, I am able to precisely identify and measure a hidden time cost of DCB policies. Using high-frequency scanner data from a national supermarket chain and an event study design, I quantify the effect of DCB policies on the wait and processing time of checkout services provided by supermarkets. My results show that DCB policies cause a persistent 3% increase in transaction duration. Moreover, given the capacity constrained queuing system of supermarket checkout, the 3% slowdown of individual customers compounds into an even larger congestion externality—with DCB policies leading to an average additional minute of wait and processing time per customer. The policy implications of my results are threefold. First, policies which incentivize consumers to change their habits may have large non-monetary costs, and ignoring these costs overstates the welfare gains of such policies. Second, not fully considering the institutional constraints of a policy setting can result in competing externalities. I show that when consumer behavior is connected through queuing systems, individually slower actions propagate into an even larger congestion externality. Third, the policy tool (i.e., bag bans vs. bag fees) matters with respect to the time costs. I find that policies which tax both plastic and paper bags have less persistent time costs than policies which ban plastic and tax paper, due to paper bags being a slower technology.

The third chapter, ***Bag "Leakage": The Effect of Disposable Carryout Bag Regulations on Unregulated Bags***, examines whether DCB policies have unintended consequences that undermine the benefits of the policies. In particular, do DCB policies lead to increased consumption of other types of plastic bags? In California, DCB policies prohibit retail food stores from providing customers with thin plastic carryout bags at checkout and require stores to charge a minimum fee for paper carryout bags. However, all remaining types of disposable bags are unregulated (e.g., garbage bags, food storage bags, paper lunch sacks). Using quasi-random variation in local government DCB policy adoption in California from 2008-2015, I employ an event study design to quantify the effect of bag regulations on the consumption of plastic and paper carryout bags, as well as the consumption of other disposable bags sold. This article brings together two data sources: (i) weekly retail scanner data with product-level price and quantity information from 201 food stores in California, and (ii) observational data collected at checkout in seven Californian supermarkets. The main results show that a 40 million pound reduction of plastic from the elimination of plastic carryout bags is offset by an additional 16 million pounds of plastic from increased purchases of garbage bags (i.e., sales of small, medium, and tall garbage bags increase by 67%, 50%, and 5%, respectively). This plastic bag "leakage" is an unintended consequence of DCB policies that offsets the benefit of reduced plastic carryout bag use. Additionally, DCB policies lead to a 69 million pound increase in paper carryout bags used annually. Altogether, I show that DCB policies are shifting consumers towards fewer but heavier bags. This chapter concludes by discussing the environmental implications of policy-induced changes in the composition of plastic and paper bags, with respect to carbon footprint, landfilling, and marine pollution.

While this dissertation focuses on one set of policies, the results have implications for any policy intervention where consumers are incentivized away from behaviors with negative externalities. Combining the lessons in all three of my dissertation essays, I show that both bans and fees are effective policy tools for changing behavior, however, changing behavior comes at the expense of time and effort. Moreover, bans and fees can be easily circumvented if not all products in the market are regulated. Future research should continue to look at the tensions between consumer behavior and the environment. Two open questions are: With advances in supply chains and the rise of online ordering of groceries, (1) how do changes in convenience alter where and how consumers acquire food, on average and heterogeneously by socioeconomic status, and (2) what are the trade-offs between this increased convenience and the environment?

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Acknowledgments

I am indebted to Sofia Berto Villas-Boas, Peter Berck, Hilary Hoynes, and David Zilberman for their continuous encouragement and guidance. I gratefully acknowledge the helpful comments and suggestions of Michael Anderson, Maximilian Auffhammer, Ceren Baysan, Kendon Bell, Joshua Blonz, Fiona Burlig, Lee Clemon, Aluma Dembo, Meredith Fowlie, Angeli Kirk, Daley Kutzman, Jeff Perloff, Louis Preonas, John Schindler, Andrew Stevens, Daniel Tregeagle, Erin Wolcott, Yang Xie, the participants of the UC Berkeley ERE seminar and the WEAI Graduate Student Dissertation Workshop, and the many others who have provided useful input. I thank Kate Adolph, Isabel A. Brito, Katherine Cai, Samantha Derrick, Tess Dunlap, Valentina Fung, Claire Kelly, Ben Miroglio, Nikhil Rao, Lucas Segil, Corinna Su, Edwin Tanudjaja, Vasco Villas-Boas, and Sarah Zou for their superb research assistance. This project would not be possible without the institutional and technical support of the retailers that provided data and access to their stores. AC Nielsen scanner data calculated based on data from The Nielsen Company (US), LLC and marketing databases provided by the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. This project was conducted with generous support from the National Science Foundation Graduate Research Fellowship Program and the Giannini Foundation. All errors are my own.

I dedicate this dissertation to my partner Mickey for his love and support, to my family for their unwavering encouragement, and to a certain supermarket in Washington, DC whose serpentine checkout line first inspired this project.

Chapter 1

Bans vs. Fees: Disposable Carryout Bag Policies and Bag Usage

1.1 Introduction

Disposable carryout plastic bags bring convenience to supermarket customers, but at a substantial cost to the environment and to municipalities trying to keep their streets and waterways clean.[1] With plastic bag clean-up, recycling, and land-filling costing cities and local governments millions of dollars per year, disposable carryout bag (DCB) policies are gaining popularity among lawmakers across the county.[2] DCB policies prohibit retail stores from providing customers with free carryout bags at checkout. The ultimate goal of these policies is to alter consumer behavior—that is, curbing the consumption of single-use bags and encouraging the use of reusable bags. Yet, while all DCB policies share this common goal, in practice they differ in their policy prescriptions, and can be divided into two competing approaches: (1) bag bans—command-and-control approaches to regulate behavior directly; and (2) bag fees—market-based approaches to incentivize individuals to change their own behavior.

The adoption of both types of policies has been widespread in the U.S. At the local level, fees on both plastic and paper carryout bags have been adopted by several cities and counties on the east coast, while the most popular policy prescription on the west coast has been bans on plastic bags coupled with fees on paper bags. At the state level, as of August 2015 two states have banned plastic bags, at least fifteen states have considered bag bans, and at least

[1]A published version of this chapter can be found in the journal of *Applied Economic Perspectives and Policy* (Taylor and Villas-Boas, 2016b).

[2]For lists of DCB policies by city, county, and state (and by adoption/rejection date), we recommend the following online resources: BagLaw.com and Californians Against Waste, *accessed Apr. 25, 2017.*

eighteen states have considered bag fees.[3,4] In light of the pervasiveness of both policies, this paper asks: Does the policy tool matter with respect to changing consumer behavior?

The standard economic analysis, dating back to Pigou (1920), has emphasized the efficiency advantages of economic incentive (EI) mechanisms such as fees and taxes, over command-and-control (CAC) approaches such as bans (Sandmo, 1978). When polluters (i.e., plastic bag users) differ in their costs of abatement (i.e., forgoing plastic bags), the flexibility offered by EI approaches reduces the aggregate cost of achieving a given level of pollution reduction compared to uniformly-applied CAC policies. However, the cost of information and monitoring also underlie the choice between taxes and bans. Compliance with bans may be relatively cheaper to monitor, which could account for the occurrence of these forms of regulation (Christiansen and Smith, 2012). Moreover, as Weitzman (1974) illuminates in his seminal work "Prices vs. Quantities," when there is significant uncertainty about the costs of pollution abatement, the outcomes from regulations that set a pollution quantity (or cap) can differ from those which set a pollution price, and conditions for one to dominate depend on the sensitivity of marginal abatement costs and marginal pollution damages to the pollution level. Therefore, if policymakers do not know consumers' demand elasticity for disposable carryout bags or the pollution damages from an additional bag, economic theory is ambiguous about whether bans or taxes should be chosen.

Given the theoretical uncertainty and the growing prevalence of both types of policies, it is imperative to compare outcomes under each regulation tool. With this objective in mind, first, we empirically investigate whether a bag ban has had its intended effects on the types of bags consumers use at checkout, and second, we compare and contrast outcomes under this bag ban to those of a bag fee. To accomplish the first task, we examine a policy change that occurred in the neighboring Californian cities of El Cerrito, Richmond, and San Pablo. Starting January 1, 2014, these cities began to prohibit retail stores from providing customers with plastic single-use bags at checkout. Any retail establishment that provides a recycled paper carryout bag or reusable bag to a customer must charge a minimum of five cents per bag. Using this policy change as a natural experiment, we collected observational data on customers' bag choices at a set of stores in these cities, both before and after the policy took effect. We observed customers during checkout and recorded the number and types of bags used, the length of transaction times, whether a bagger was present and basic customer demographic information. Besides collecting counterfactual data in the pre-policy period, we also collected data at stores in the control cities of Berkeley and Concord, where there was no bag policy change during our sample period.

With our unique panel dataset, we use a difference-in-differences empirical strategy to measure how plastic bag bans with paper bag fees affect customers' demand for various types of disposable and reusable bags. We also measure whether changes in consumer behavior due

[3] All four counties in Hawaii have banned plastic bags, making it a statewide ban in effect. In California, a statewide ban on plastic bags passed the state legislature and was signed into law by the governor on September 30, 2014. However, opponents secured enough signatures to put the ban to a public vote, meaning the ban is effectively on hold until November 2016.

[4] In addition, a handful of states are considering laws that would ban cities from banning plastic bags.

to the policy shock are heterogeneous across fee size and store type. In particular, we collect data at two markedly different grocery chains within the same treated and control cities. Not only do these chains attract a different clientele within the same city—as evidenced by the demographic data we collect—their management also chose different responses to the same DCB policy. The first chain, which we refer to as the National Chain, chose to charge the minimum required five cents per paper bag. The second chain—referred to as the Discount Chain—chose to charge ten cents per paper bag and introduced a 15-cent thick-plastic reusable bag. By running the same analysis on each of these chains separately, we are able to compare how the types and prices of bags offered influence consumer behavior.

For our second objective, we investigate how outcomes under plastic bag bans compare to outcomes under bag fees by juxtaposing our analysis with a study on a concurrent bag fee: Homonoff's (2016) investigation of the 5-cent plastic and paper bag fee in Montgomery County, Maryland. While previous studies exist that examine how DCB policies affect consumer behavior (e.g., Dikgang et al. 2012), Homonoff (2016) was the first to collect data on bag usage in both the pre- and post-policy period and at treated and control stores. By replicating the methodology of Homonoff (2016) on our sample of stores, we are able to directly compare results, and consequently draw conclusions on the effectiveness of bag bans versus bag fees.

The empirical literature comparing outcomes under bans and fees is limited, especially with respect to consumption-driven externalities. Adda and Cornaglia (2010) analyze these two policies on passive smoking and find that excise taxes are more effective at reducing exposure to tobacco smoke than smoking bans in public places. The reason is that bans displace smokers to private places where they contaminate non-smokers, especially young children. Although disposable carryout bags and cigarettes are very different commodities in many respects, our analysis likewise finds that bans displace consumption in unintended ways in which taxes do not.[5]

To preview some of our results, at the National Chain we find that both policies lead to remarkably similar increases in reusable bag usage and reductions in disposable carryout bag usage.[6] However, under a plastic bag ban, the eradication of plastic bag consumption is offset by a 46.50 percentage point increase in paper bag consumption. In comparing the Discount Chain to the National Chain, we find that charging ten cents (instead of five cents) for paper bags and offering inexpensive, 15-cent reusable bags leads to larger reductions in total disposable carryout bag usage. In particular, after the policy change, paper bag demand increases by only 10.12 percentage points at Discount Chain stores. Thus, the types of reusable bags that stores decide to sell in lieu of single-use plastic bags—as well as the price of these alternatives—have significant impacts on the effectiveness of bans versus fees, especially with respect to paper bag demand.

[5] Other empirical studies have compared EI instruments versus CAC approaches in reducing air pollutants (Oates et al., 1989) and in incentivizing the production of higher fuel economy vehicles (Greene, 1990).

[6] We refer to both paper and plastic bags as disposable carryout bags. Thus, for customers facing a plastic bag ban, disposable carryout bag usage is only comprised of paper bags. For customers facing a paper and plastic bag fee, it is a mix of both.

This study uniquely contributes to the literature by being the first to rigorously analyze consumer responses to plastic bag bans and the first to compare outcomes under bag bans to those under bag fees. We also extend the analyses of previous studies by measuring the use of all subcategories of carryout bags—as opposed to broad categories—and by investigating the heterogeneity of effects by store type. As such, the results of this paper have the potential to shape current and future policy—and not just policies regulating disposable carryout bags, but also the regulation of other disposable products (e.g., Styrofoam containers and plastic bottles). Our results suggest that as cities, counties, and states continue to design and adopt DCB policies, they will want to consider not only their objectives for reusable and plastic bag usage, but also their objectives for paper bag consumption.

1.2 Background on Disposable Carryout Bag Policies

Standard single-use plastic bags cost retailers an average of three cents each and paper bags cost seven to ten cents each.[7] However, customers do not directly see the price of the disposable carryout bags they use, as the majority of grocery stores roll the price of these bags into the total cost of the transaction. Without seeing a price, customers are prone to use more plastic bags than they would be willing to pay for at face value, and this overuse is costing municipalities millions of dollars in clean-up expenses. Each year Americans are estimated to consume 100 billion single-use plastic bags (Clapp and Swanston, 2009)—approximately 325 bags per person per year—the vast majority of which are eventually landfilled or littered. When not kept in landfills, plastic bags end up in storm drains, rivers and oceans—degrading water quality, harming birds and aquatic life, clogging waterways, and becoming a general eyesore. Even when properly disposed of in a receptacle, plastic bags are easily blown away due to their light weight and aerodynamics. Moreover, the 13.5% of plastic bags that are recovered (US EPA 2015) become a major problem for recyclers, as the bags often clog the machines used to sort material.[8] A study of the budgets of six major cities in the United States finds that the litter control of plastic bags costs between 3.2 and 7.9 cents per bag.[9] Given that approximately 100 billion plastic bags are consumed in the United States each year, municipalities nationwide spend between \$3.2 to \$7.9 billion per year to clean up plastic bags.

In light of the sizeable costs of plastic bags, one might be tempted to answer the age-old question "Paper or Plastic?" with a resounding "Paper!" However, single-use paper bags are not without their own environmental costs and thus may not be an adequate alternative

[7] Bag cost estimates come from interviews with the store managers in our sample, but media articles also confirm these estimates ("Plastic Ban Means Higher Costs are in the Bag." *Crain's Chicago Business*. May 3, 2014. Online, *accessed Apr. 25, 2017*). The cost of paper bags depend on bag size and the presence of handles. Store owners list disposable carryout bags as their fourth largest operating cost, after electricity, payroll, and credit card fees.

[8] "Which Bag is Best: Paper or Plastic?" *The Oregonian*. May 17, 2007. Online, *accessed Apr. 25, 2017*.

[9] "Do Bans on Plastic Grocery Bags Save Cities Money?" *National Center for Policy Analysis*. Dec. 2013. Online, *accessed Apr. 25, 2017*.

to plastic. The negative environmental impacts of paper bags include: paper bags are more energy and water intensive to manufacture than plastic bags; paper bag production generates 70% more air and 50 times more water pollutants than the production of plastic bags; it takes 98% less energy to recycle a pound of plastic than a pound of paper; and paper bags are 9 times heavier than plastic bags, requiring more space in transportation trucks and landfills.[10] By switching from plastic to paper, society is simply trading one set of environmental costs for another. However, the important take-away is that the environmental costs of single-use plastic bags are felt acutely in the budgets of local municipalities, especially those along waterways, whereas the environmental costs associated with paper bags are more broadly spread across all people in a population. Therefore, it is unsurprising that in practice DCB policies have been more aggressive against plastic bag consumption than paper bag consumption.

Policies to Regulate Disposable Carryout Bags

Given the aforementioned costs of disposable carryout bag usage, governments in the United States began passing laws to regulate disposable carryout bags in the mid to late 2000s.[11] On January 1, 2010, Washington DC became the first city in the United States to require a fee, charged at checkout, for using single-use plastic and paper bags. This law—which passed in response to a District Department of Environment study that found plastic bags comprised 47% of the trash in Washington DC's rivers and tributaries (DDOE 2008)—had the joint goals of changing the consumption of bags and raising revenue for watershed clean-up. Several counties surrounding Washington DC have since followed suit with similar laws. Although fee magnitude has varied across municipalities (between five and 25 cents), in general retailers retain a portion of the fee to cover the cost of implementation, while the municipality may keep any remaining cents, which are to be used for funding clean-up and recycling programs.

Unlike many of their East Coast counterparts, Californian cities and counties have passed plastic bag bans with paper bag fees instead of requiring fees for both. In California, the charges for paper bags are kept completely by the retailers and none of the revenue is collected by the local government implementing the law. Why has California chosen bans over fees? The answer lies in Assembly Bill 2449 (California State Legislature 2006). This bill, enacted in 2006, began as a plastic bag fee bill, but—due to pressure from the plastic industry—transformed into a plastic bag recycling bill. Moreover, a last-minute change to this law prohibits "a city, county or other public agency from adopting, implementing, or enforcing an ordinance, resolution, regulation or rule that imposes a plastic carryout bag fee upon a store." Consequently, a plastic bag fee was not an available policy option for

[10] "Graphic: Paper or Plastic?" *The Washington Post*. Oct. 3, 2007. Online, *accessed Apr. 25, 2017.*

[11] Internationally, DCB regulations started earlier, with several countries banning the use of plastic bags in the global south, where the externalities of plastic bags are exacerbated by less-established municipal waste collection and recycling. Policies later spread to the global north, with Ireland becoming the first industrialized country to impose a tax on customers for using plastic bags in 2002 (Convery et al., 2007).

California municipalities that did not feel recycling programs went far enough in addressing their disposable carryout bag problems.[12]

San Francisco was one such city. In 2007, San Francisco enacted a citywide ban on single-use plastic bags, becoming the first government in the United States to do so. This sparked the spread of DCB policies in California, with the cities of Malibu and Palo Alto adopting similar laws in 2008 and 2009, and Los Angeles County passing even stronger legislation in the summer of 2011—not only banning plastic bags but also charging a minimum of ten cents per paper bag. By the time the Richmond bag ban (Richmond Municipal Code 2013)—the primary policy in our analysis— went into effect in 2014, over 55 city and county bans had been implemented in California.[13] While the requirements of Assembly Bill 2449 were only operative until January 1, 2013 and any bills passed in California in and after 2013 could technically choose bag fees over bag bans, interestingly none of the ordinances passed in 2013 or 2014 opted for a plastic bag fee.[2]

Yet plastic bag fees are not completely out of the picture. In 2013 two bills were introduced in the California Senate—one of them a plastic bag ban with a paper bag fee and the other a fee on both plastic and paper bags.[14] Furthermore, several supermarket chains in areas affected by plastic bag bans have found a way to satisfy customers willing to pay for plastic bags—selling 15-cent thick-plastic reusable bags at checkout. These bags meet all the requirements of current Californian DCB policies: they are at least 2.25 mils thick, they are capable of carrying 22 pounds for 125 uses, they have handles and are made from materials that can be cleaned or disinfected, they are made from 20% recycled materials and are recyclable themselves. The main issues with the current single-use plastic bags are that they are "free"—leading them to be excessively overused—and lightweight—making them difficult to landfill and recycle. Selling a bag that is neither free nor lightweight solves both of these issues, while also providing utility to customers willing to pay for them.[15]

However, some view these 15-cent bags as a loophole for stores and manufacturers to get around the intention of the plastic bag bans. Skeptical on whether 15-cent thick-plastic bags are in fact reused, a handful of municipalities are considering upping their reusable bag requirement to 3 mils thick to prevent the use of these bags.[16] Therefore, not only do governments considering DCB policies have to decide between bag bans versus bag fees, they must also decide on the types and thickness of reusable bags to allow. Our paper addresses both policy debates, first by comparing consumer demand outcomes under bag bans to those under bag fees, and second by examining the various types of bags that consumers reuse—

[12] "The Plastic Bag Ban Epic." *LA Observed*. Sep. 6, 2014. Online, *accessed Apr. 25, 2017*.

[13] Another 50 local bag bans were adopted by the time the statewide ban was signed into law in September 2014.

[14] Senate Bill 270 (California State Legislature 2014)—the bill that was signed into law—bans single-use plastic carryout bags statewide and requires stores to sell paper and reusable bags for at least ten cents each. Senate Bill 700 (California State Legislature 2013)—which died in committee—would have instead required stores to charge five cents for both plastic and paper carryout bags.

[15] Senate Bill 270 also provides $2 million in grant money from a California recycling fund to assist plastic bag-making businesses in producing these types of reusable bags.

[16] "Industry Finds Bag Ban Loophole." *ecoRI News*. Jun. 9, 2014. Online, *accessed August 26, 2015*.

whether they are standard reusable tote bags, paper bags, backpacks, boxes, or alternative 15-cent reusable bags.

1.3 Data Collection and Summary Statistics

To assess the effectiveness of single-use plastic bag bans, we take advantage of a policy change in the neighboring Californian cities of El Cerrito, Richmond, and San Pablo. Starting on January 1, 2014, these cities began to prohibit retail stores from providing customers with plastic single-use bags at checkout. Now, any retail establishment that provides a recycled paper carryout bag or reusable bag to a customer must charge the customer a minimum of five cents for each bag provided, a fee that goes entirely to the retailer collecting it. Using this policy change as a natural experiment, we collect observational data on customers' bag choices at a set of grocery stores, including three treated stores located in the cities of El Cerrito, Richmond, and San Pablo, two control stores in the city of Berkeley (where a DCB policy had already been imposed a year prior to the new policies), and two control stores in the city of Concord (where no DCB policy has been enacted). Since the city of Richmond geographically envelopes the cities of San Pablo and El Cerrito, we group all three cities together as one region (Richmond) for the remainder of this paper.

The data were obtained through direct observation of transactions by members of our research team stationed near checkout lanes. For each transaction we collected data on the number and types of bags used, whether a bagger was present, the length of the transaction, and basic demographic data such as gender and race of the person paying. This type of transaction specific information can only be gained from in-store observations, and is not included in the scanner datasets from these stores. Four visits per store occurred in November and December 2013, before the Richmond DCB policy went into effect, and 4–6 visits occurred in January and February 2014, after the ban was in place. We also made an additional four visits in March and April 2014 to collect follow-up data. Each visit lasted 1–2 hours and was made on either Saturday or Sunday between 11am–7pm—high foot-traffic hours for grocery shopping.[17]

Our methodology for data collection is based on that of Homonoff (2016) in that we collect data by direct observation at treated and control stores in the pre- and post-policy period. However, while Homonoff (2016) collected data on bag usage in broad categories (disposable carryout bags versus reusable bags), we go further and collect data on all subcategories of carryout bags. Subcategories of disposable carryout bags include single-use plastic and single-use paper. Subcategories of reusable bags include reusable tote bags; 15-cent thick-plastic bags; reused disposable paper and plastic bags; backpacks; purses; boxes; suitcases; and other.We collect such data in order to measure changes in bag usage within reusable and disposable categories (i.e., plastic versus paper bags, 15-cent versus one-dollar reusable bags), which are an important consideration for both store operators and policymakers evaluating

[17]To prevent potential biases, we randomized the order in which we visited the stores on each observation date.

consumer behavior and policy success. This paper also extends the work of Homonoff (2016) by collecting data on the presence of baggers—which may influence the number and types of bags customers use—and by collecting data at two distinct types of grocery chains—which allows us to examine whether the effects of DCB policies are heterogeneous by store type.

We visited a total of seven stores belonging to two different categories of grocery chains within the same treated and control cities. The first chain is a large national chain, offering high and low prices in many products, located in very diverse demographic regions, and that, in the pre-period, offered to its consumers at checkout free plastic bags, free paper bags, and reusable tote bags for purchase at $1.50. The other chain we collected data from is a regional discount chain, which in the pre-period offered only free single-use plastic bags and reusable tote bags for purchase at $0.99. Not only did these two chains offer different types of bags in the pre-period, their respective management also responded differently to the same policy change. The National Chain reacted to the ban by continuing to offer the same paper and reusable tote bags as before the policy, but added the required 5-cent fee per paper bag. The Discount Chain retained the reusable totes it sold before the ban as well, and also introduced a ten-cent paper bag and a 15-cent thick-plastic reusable bag.[18] Besides charging the minimum amount for paper bags required by law, the National Chain's paper bags are of a slightly better quality than the Discount Chain, whose paper bags do not have handles. Table 1.1 reports background information on the seven stores in our sample—three from the National Chain and four from the Discount Chain—including when the policy went into effect at each store, what stores charge for paper bags in the post-policy period, the number of transactions we observed at each store, and pre-ban store characteristics (average number of lanes open, percentage of transactions with baggers present, and average length of transaction time in seconds).[19] At the National Chain, the policy change produces an infinite price on plastic since plastic is no longer available, a five-cent increase in the price on paper, and no change in the price on reusable tote bags. In this case we can see how consumers react to the differential relative price changes of the bag options available to them. At the Discount Chain, we instead capture how consumers suddenly adjust behavior to new bag alternatives that have a positive price as compared to the free plastic option. Moreover, by measuring the effects on consumer behavior in the Discount Chain separately from the National Chain, we were able to focus on what we hypothesize to be a very price-sensitive population.[20]

[18]The price of the reusable tote bags does not change after the policy implementation at either chain. While the Discount Chain sells $0.99 reusable totes before and after the ban, our team never witness a purchase of these bags.

[19]More observations were made at National Chain stores on average over the sample period because data collection occurred at the National Chain stores on two dates more than at the Discount stores. Also, since the Richmond area has two locations of the Discount Chain, we decided to make observations at both in order to increase our total number of observations and statistical power.

[20]While we cannot measure the income of customers in our sample, based on our conversations with managers and anecdotal evidence at the Discount Chain stores, we conjecture that Discount Chain stores have a higher proportion of low income customers and SNAP and WIC participants than the National Chain.

Table 1.1: Bag Ordinances, Number of Transactions Observed, and Average Pre-Ban Transaction Characteristics

	Bag Ordinances		Number of Txns. Observed				Avg. Pre-Ban Statistics		
			2013	2014					
	Bag Ban Start Date	Paper Bag Fee	Nov-Dec	Jan-Feb	Mar-Apr	Total	Lanes Open	Share Txns. w/Baggers	Txn. Duration (seconds)
National Chain									
Berkeley	Jan. 1, 2013	10 cents	226	272	199	697	4.67	0.54	92.98
Richmond	Jan. 1, 2014	5 cents	200	271	194	665	4.59	0.65	88.93
Concord	No Ban	"Free"	254	321	199	774	4.68	0.51	86.62
Total Obs.			680	864	592	2136			
Discount Chain									
Berkeley	Jan. 1, 2013	10 cents	156	163	201	520	3.40	0.08	95.56
Richmond	Jan. 1, 2014	10 cents	433	341	384	1158	2.28	0.43	75.52
Concord	No Ban	"Free"	199	183	228	610	2.95	0.56	86.11
Total Obs.			788	687	813	2288			

Note: We observed transactions at 7 stores: 3 in the National Chain = {Berkeley (x1), Richmond (x1), and Concord (x1)} and 4 in the Discount Chain = {Berkeley (x1), Richmond (x2), and Concord (x1)}. *Source*: Authors' calculations from observational data collected in-store at checkout.

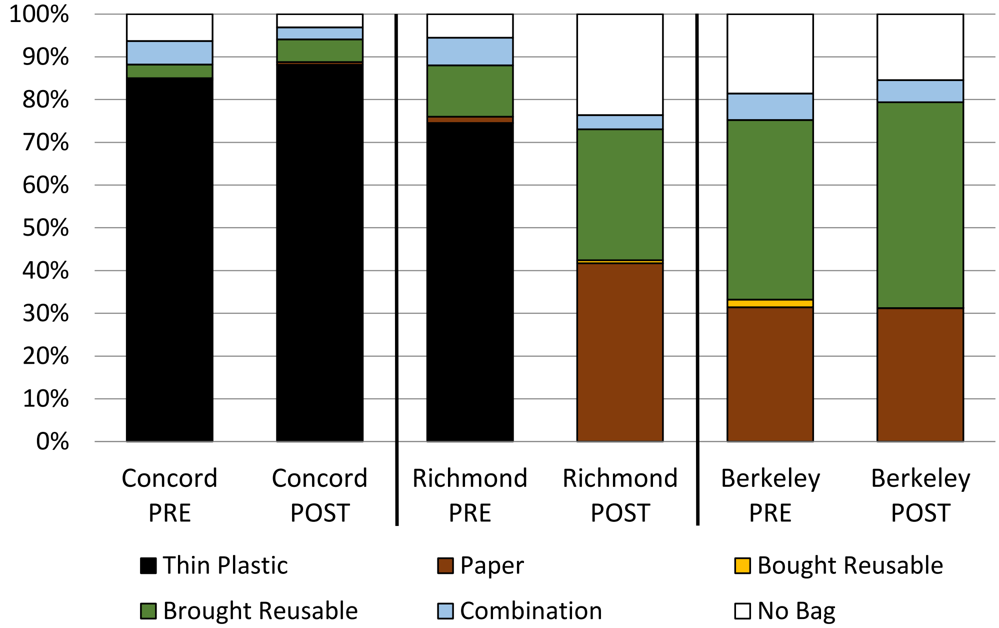
Plotting the raw means of the bag usage data speaks volumes. Figures 1.1a and 1.1b plot, for the National Chain and the Discount Chain stores, respectively, the percentage of transactions using each of six types of bags: (i) thin, single-use plastic bags; (ii) paper bags; (iii) reusable bags bought during checkout; (iv) brought reusable bags; (v) a combination of multiple bag types; and (vi) no bags. The summary statistics are separated into three policy regions: Concord (control stores with no ban ever); Richmond (treated stores with a policy change); and Berkeley (control stores with a ban pre-dating the sample period). Each of these cities is further examined in two time periods: the PRE-ban period (Nov–Dec 2013) and the POST-ban period (Jan–Feb 2014). In figure 1.1, we see stark changes in the distribution of the type of bags used that are contemporaneous with the bag policies across the treatment and control groups. The first thing to note is that only the treated Richmond stores see a noticeable change in bag usage between the PRE and POST periods. Second, while the Richmond stores have similar bag use distributions to their Concord counterparts in the PRE period, they switch to a distribution more similar to that of their Berkeley counterparts in the POST period. The take-away of these graphs is that stores in cities with plastic bag bans and paper bag fees see total disposable carryout bag usage decrease (plastic plus paper), however, paper bag usage increases markedly. The policy also leads to higher use of reusable bags and higher incidence of customers using no bags at all.

We can also examine how the distributions of bag usage vary across store type, for stores with bans, and for stores without. At both the National Chain and Discount Chain stores, roughly 95% of transactions use some sort of bag when there is no bag policy in place. Conversely, only 80% of transactions at National Chain stores use a bag when there is a bag policy, and only 70% do likewise at Discount Chain stores. With regard to reusable bags, between 30% and 50% of transactions bring reusable bags to stores that have bag policies in effect. This is true for both chains. In terms of purchasing bags, at National Chain stores with bans, 40% of transactions pay the five cents per paper bag with handles, and only 1% pay $1.50 for a reusable tote bag. At Discount Chain stores, only 30% of transactions pay for bags at checkout, with approximately 10% paying ten cents for paper and 20% paying 15 cents for thick-plastic reusable bags. Interestingly, the 15-cent bag is more popular than the 10-cent paper bag at Discount Chain stores. This preliminary surveillance of the raw data suggests that plastic bag bans cause a large increase in paper bag demand; however, this increase in paper demand may be mitigated if customers are charged more for paper bags or offered an inexpensive and desirable alternative.

Next we use the pre-treatment period data to investigate whether the pre-period is balanced in terms of observable determinants, such as customer demographics and bag usage within and across chains. Since we will employ a difference-in-difference strategy, a critical assumption is the appropriate selection of the treatment and control groups. Our selection rules for the control and treated cities were as follows: (1) the presence of both retail chains within the city; (2) proximity—only cities within one hour of travel for the research team; (3) accessibility by public transit; and (4) most importantly, the two control cities best matched to Richmond in terms of average demographics characteristics, based on available census data. To verify whether the control and treatment cities are well-matched, table 1.2 presents

Figure 1.1: Percentage of Transactions Using Each Bag Type

(a) **National Chain**



(b) **Discount Chain**

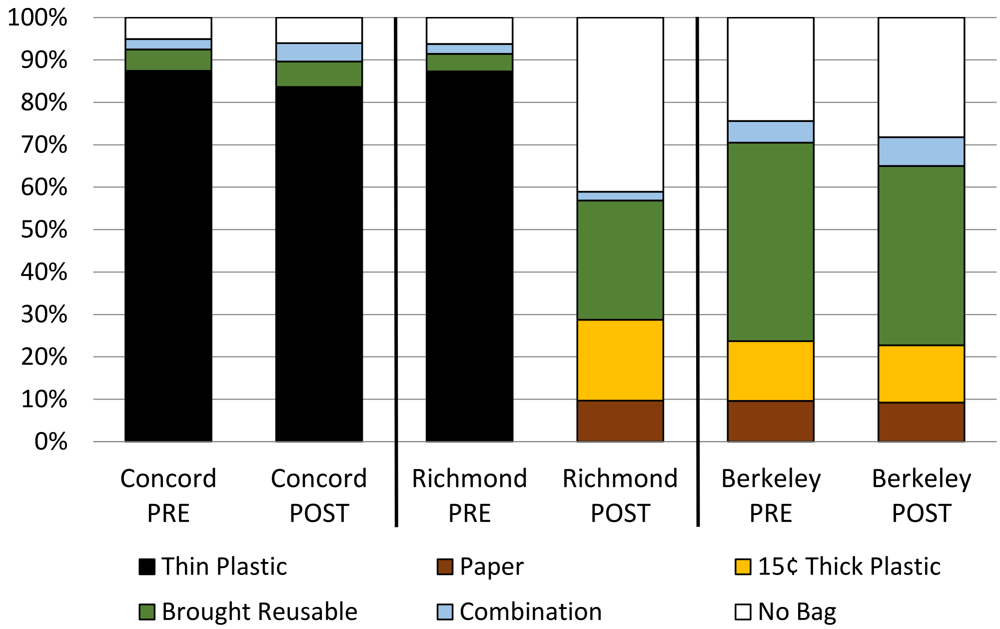


Table 1.2: Demographics, Pre-Ban

	Berkeley	**Richmond**	**Concord**
2012 Census Data			
Male	0.489	0.489	0.498
White	0.627	0.497	0.704
Black	0.088	0.212	0.038
Under 18	0.130	0.243	0.226
Population	122,662	157,176	122,683
Avg. Salary	$42,332	$28,365	$33,511
National Chain (Observed Data)			
Male	0.469	0.410	0.461
	(0.500)	(0.493)	(0.499)
White	0.690	0.580	0.764
	(0.463)	(0.495)	(0.426)
Black	0.106	0.250	0.0433
	(0.309)	(0.434)	(0.204)
Discount Chain (Observed Data)			
Male	0.532	0.406	0.442
	(0.501)	(0.492)	(0.498)
White	0.449	0.293	0.613
	(0.499)	(0.456)	(0.488)
Black	0.308	0.346	0.146
	(0.463)	(0.476)	(0.354)

Note: Standard deviations in parentheses. Table reports mean values of each variable in the pre-period. *Sources*: In-store observational data & U.S. Census Bureau, 2008-2012 American Community Survey 5-Year Estimates. Richmond census values are population weighted averages of Richmond, El Cerrito, and San Pablo.

demographic and income summary statistics from the pre-period. Reported in the top panel are 2012 census data for the proportion of each city's population that is male, white, black, as well as the total population and the average salary in each city. Richmond has a larger proportion of minority residents and a lower average salary than either Berkeley or Concord. To understand whether our selected stores are representative of the entire city, the middle and bottom panels present the summary statistics of the observational data collected for the proportion of customers that were male, white, and black at the National Chain and

Discount Chain stores, respectively. Compared to the census data, the clientele of the National Chain stores appear to be fairly representative of the cities in which they live. On the other hand, the percentage of black and minority clientele at the Discount Chain stores are 10–20 percentage points greater than representative of the census data, and this is true across all cities. We contend this is strong evidence that the National Chain and Discount Chain stores attract a very different clientele, even within the same city.

The other critical identification assumption is that of parallel trends—where, had it not been for the DCB policies, Richmond would have had the same change in bag usage over time as Berkeley and Concord. To validate this assumption, we use the pre-policy data and regress each of the bag usage variables on a time trend, an indicator for treatment, and their interaction. We find the point estimates of the time trend are not statistically different between treatment and control stores, with the exception of paper bags at the treated National Chain, which see a downward trend before the ban went into effect. Since the hypothesis is that plastic bag bans lead to an increase in paper bag usage, we would be more concerned if the opposite were true—that is, an upward trend in paper bag usage in the treated stores before the policy.

With regard to other observable characteristics, the last three columns of table 1.1 show that, in the pre-period, National Chain stores have baggers present for between 51–65% of transactions, approximately 4.6 registers open at a time, and transaction times ranging from 86–93 seconds. Conversely, the Discount Chain stores have baggers present less frequently (8–56% of transactions), fewer registers open (2.3–3.4 registers), and a wider variance of average transaction times (76–96 seconds). Overall, the sample averages for these variables compared within each chain are similar, suggesting that the treatment and control stores share broadly equivalent patterns in the pre-period.[21]

1.4 Difference-in-Differences Estimation Strategy and Results

We use a regression framework to evaluate the effect of the Richmond bag ban on measures of bag demand controlling for various individual- and store-level covariates. We consider two measures of bag demand: demand on the *extensive margin* (the percentage of customers using each type of bag), and demand on the *intensive margin* (how many bags each customer uses, given that they use a particular type of bag). We exploit the quasi-experimental panel design to employ a difference-in-differences estimation strategy. The reduced form specification is:

$$Y_{st}^B = \beta_1(Richmond \times Post)_{st} + \beta_2 Post_{st} + \alpha_s + \gamma X_{st} + \epsilon_{st} \tag{1.1}$$

where Y_{st}^B is a measure of demand on either the extensive or the intensive margin in store s during year t for type of bag $B = \{$plastic, paper, bought reusable, brought reusable, no bag$\}$,

[21]The Berkeley Discount store appears to be an outlier across these 3 measures in the pre-period. This is partly because the Berkeley store already had a bag policy during the pre-period and partly due to the layout of the checkout lanes at the Berkeley store, which has very little standing room for baggers.

$Richmond_s$ is an indicator for observations at stores in Richmond, $Post_t$ is an indicator for observations in 2014, which are after policy implementation, as represents store fixed effects, and X_{st} is a set of controls. The coefficient of interest is β_1, that is, the coefficient on the interaction of $Richmond_s$ and $Post_t$, which measures the effect of the ban on demand at treated stores in Richmond relative to changes in demand at control stores in Berkeley and Concord. We estimate the above specification for each chain and bag type separately.

Tables 1.3 and 1.4 present the results—from the National Chain and Discount Chain stores, respectively—for the effects of the Richmond bag ban on bag demand for the various types of bags, along both the extensive and intensive margins. To estimate demand on the extensive margin, we use a linear probability model where the outcome variable is the probability of using a particular type of bag.[22] To estimate demand on the intensive margin, we use OLS where the outcome variable is the total number of bags used among those that use them. Each specification includes—in addition to the interaction of the Richmond*POST indicators, the POST indicator, and store fixed effects—fixed effects for the date of visit, hour of visit, week-in-month of visit (first, second, third, fourth), and observer. We include these fixed effects since the profile of the average grocery shopper is not constant over the course of a day, day of the week, or week in the month.[23] We also control for whether the transactions occurred in an express lane, whether a bagger was present for at least part of the transaction, whether the customer paying was male, and whether the customer paying was white. These controls are added to further prevent differences across locations and time periods from biasing the results. Lastly, we cluster the standard errors at the store-day level to account for the possibility that the errors are correlated within a given store on a particular day, but not across stores or dates.

In table 1.3 we find that, with regard to the extensive margin, the ban at National Chain stores led to a decrease in plastic bag consumption of 81.57 percentage points, an increase in paper bag use of 46.50 percentage points, an increase in bringing reusable bags of 26.03 percentage points, and an increase in buying reusable bags of 4.24 percentage points. In addition, the percentage of customers using no bags increased by 9.25 percentage points. Adding the β_1 coefficients from columns (1) and (2) shows that total usage of disposable carryout bags (paper plus plastic) decreased by approximately 35 percentage points.[24] With

[22]We found that a probit model produces similar results.

[23]Studies have shown that as the day progresses, the average age of grocery shoppers declines while the average income rises, and that shoppers on Sundays are noticeably younger than those on other days of the week ("Grocery Shopping: Who, Where, and When." *The Time Use Institute*. Oct. 2008. Online, *accessed Apr. 25, 2017*). Another way in which customers' profiles are not constant over time is that SNAP and WIC participants are more likely to use their benefits in the first weeks of the month. According to the Food and Nutrition Service of the USDA (2012), 80% of SNAP benefits are used within the first 2 weeks of issuance. Therefore, if age and income are correlated with using reusable bags over disposable bags and we do not include these time fixed effects, differences in the timing of observations across locations could bias our results.

[24]Since it is possible for National Chain shoppers to use both plastic and paper during a transaction, we also run equation (1) with the percentage of customers using either paper or thin plastic as Y_{st}^B. In this specification, the estimated β_1 is –33.20 percentage points, which is very close to what we obtain by simply

respect to the intensive margin, in column (6) we see that the bag ban caused the number of paper bags used per transaction by paper bag users to decrease by 2.0765 bags. Therefore, even though National Chain customers use paper bags 46.50 percentage points more often after the policy change (extensive margin), those who use paper bags use fewer paper bags per trip (intensive margin). Column (7) shows that the average number of reusable bags brought by reusable bag users at the National Chain did not increase by a statistically significant amount due to the ban. Thus, while the ban led more people to bring reusable bags, it did not alter the number of reusable bags used per trip. Lastly, we can combine the extensive and intensive margin estimates in a two-part model to approximate the overall effect of the DCB policy on reusable and paper bag demand (McDonald and Moffitt, 1980; Homonoff, 2016).[25] The combined estimates indicate that the policy increased the number of paper bags used by 1.76 bags per transaction and increased the number of reusable bags by 0.64 bags per transaction.[26]

Table 1.4 presents the results for the Discount Chain, where the imposition of the ban led to a decrease in disposable plastic bag usage of 89.05 percentage points, an increase in paper bag usage of only 10.12 percentage points, an increase in bringing reusable bags of 18.32 percentage points, and an increase in buying 15-cent reusable bags of 28.57 percentage points. The probability that customers use no bags increases by 29.67 percentage points. Adding the β_1 coefficients from columns (1) and (2) shows that total usage of disposable carryout bags decreased by approximately 79 percentage points. On the intensive margin, column (6) shows that the average number of reusable bags used by reusable bag users increased by 0.4041 bags after the policy change. As above, we combine the extensive and intensive margin estimates to approximate the overall effect of the DCB policy on reusable bag demand. We find that the policy increased the overall number of reusable bags by 0.28 bags per transaction.

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adding the coefficients in columns (1) and (2).

[25]The conditional expectation of demand can be decomposed into its extensive and intensive components as follows: $E[y|x] = E[y|x, y > 0] \times P(y > 0|x)$, where y represents demand and x represents the covariates. The total effect on bag demand of a change in one of the covariates is given by:

$$\frac{\partial E[y|x]}{\partial x} = \frac{\partial E[y|x, y > 0]}{\partial x} \times P(y > 0|x) + \frac{\partial P(y > 0|x)}{\partial x} \times E[y|x, y > 0]. \tag{1.2}$$

For this exercise, we evaluate $P(y > 0|x)$ and $E[y|x, y > 0]$ at the sample means of the treated stores, pre-policy.

[26]Given that the number of bags used of any given bag type is zero censored, we also estimate bag demand under a Tobit model. We find that the estimates are larger but qualitatively similar under a Tobit model compared to the combined demand model used here.

Table 1.3: Effect of Plastic Bag Ban on Bag Demand – Extensive & Intensive Margin (National Chain)

	Extensive Margin					Intensive Margin	
	(1) Plastic	(2) Paper	(3) Brought Reus.	(4) Buying Reus.	(5) No Bags	(6) Paper	(7) Brought Reus.
Post x Richmond	-0.8157***	0.4650***	0.2603***	0.0424***	0.0925***	-2.0765***	0.1999
	(0.0074)	(0.0268)	(0.0272)	(0.0050)	(0.0187)	(0.0850)	(0.3260)
Post	0.0774***	-0.1195**	-0.2693***	-0.0538***	0.2114***	-1.2558***	0.7731
	(0.0152)	(0.0497)	(0.0434)	(0.0110)	(0.0410)	(0.2433)	(1.1818)
Express	-0.0052	0.0331	-0.0720*	0.0008	0.0164	-0.3990*	-0.6286**
	(0.0235)	(0.0335)	(0.0398)	(0.0059)	(0.0253)	(0.2145)	(0.2578)
Bagger Present	0.0274*	0.0721***	0.0082	0.0016	-0.0701***	0.5557**	0.2560
	(0.0148)	(0.0239)	(0.0308)	(0.0034)	(0.0237)	(0.2128)	(0.1885)
Male	-0.0046	0.0324	-0.0870***	-0.0098**	0.0517***	0.1405	0.2243*
	(0.0154)	(0.0210)	(0.0256)	(0.0045)	(0.0167)	(0.1809)	(0.1297)
White	-0.0210	0.0004	0.1056***	0.0008	-0.0591***	0.0908	0.4776***
	(0.0147)	(0.0227)	(0.0233)	(0.0043)	(0.0174)	(0.1869)	(0.1622)
Num of Obs.	1544	1544	1544	1544	1544	328	411
Store FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observation FE[1]	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Clustered standard errors are in parentheses. Clusters are at the store-day level. Outcome variables: probability of using at least one bag or no bags (extensive) and bag demand among users (intensive). $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

[1]Fixed Effects for Date-of-Visit, Hour, Week-in-Month, Observer, and Day-of-Week.

Table 1.4: Effect of Plastic Bag Ban on Bag Demand – Extensive & Intensive Margin (Discount Chain)

	Extensive Margin					Intensive Margin
	(1) Plastic	(2) Paper	(3) Brought Reus.	(4) 15 cent Bag	(5) No Bags	(6) Brought Reus.
Post x Richmond	-0.8905***	0.1012***	0.1832***	0.2857***	0.2967***	0.4041**
	(0.0215)	(0.0167)	(0.0223)	(0.0276)	(0.0170)	(0.1827)
Post	-0.0504	-0.0188	0.1281***	-0.0138	-0.0735**	0.7524
	(0.0389)	(0.0346)	(0.0389)	(0.0466)	(0.0308)	(0.4847)
Bagger Present	0.0518***	0.0064	-0.0046	0.0169	-0.0588**	0.3969*
	(0.0175)	(0.0183)	(0.0178)	(0.0144)	(0.0218)	(0.2302)
Male	0.0103	-0.0237*	-0.0666***	0.0116	0.0448**	-0.1990
	(0.0100)	(0.0122)	(0.0226)	(0.0146)	(0.0208)	(0.1544)
White	-0.0276*	-0.0316**	0.1121***	-0.0348*	-0.0043	0.1451
	(0.0155)	(0.0121)	(0.0271)	(0.0172)	(0.0225)	(0.1091)
Num of Obs.	1475	1475	1475	1475	1475	320
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Observation FE[1]	Yes	Yes	Yes	Yes	Yes	Yes

Note: Clustered standard errors are in parentheses. Clusters are at the store-day level. Outcome variables: probability of using at least one bag or no bags (extensive) and bag demand among users (intensive). $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

[1] Fixed Effects for Date-of-Visit, Hour, Week-in-Month, Observer, and Day-of-Week.

Comparing the Discount Chain and National Chain results, we find that at both chains: (1) the presence of a bagger is correlated with an increase in the probability of customers using disposable carryout bags and a decrease in the probability of customers using no bags;[27] (2) male customers are less likely to bring reusable bags and more likely to use no bags than female customers; and (3) white customers are more likely to bring bags than customers of other races. When contrasting the Discount Chain and the National Chain, it is interesting to note that the increase in paper bag usage is more than 4 times larger at National Chain stores than at Discount Chain stores. Also, Discount Chain customers are 3 times more likely to use no bags than National Chain customers. Finally, customers at Discount Chain stores choose to buy 15-cent reusable bags more often than ten-cent paper bags. We suggest that these differences between chains occur for several reasons: (1) the National Chain stores sell paper bags for five cents less than the Discount Chain stores; (2) Discount Chain stores sell an alternative reusable bag for $0.15 as opposed to the National Chain, which only sells $1.50 reusable tote bags; and (3) Discount Chain shoppers may be more price sensitive than National Chain shoppers.

Analysis of Bag Demand over Time

Using the additional data collected in March and April, we look at whether bag demand changes over time, as customers grow accustomed to the Richmond bag ban. Using the same model specifications as in Tables 1.3 and 1.4 for demand on the extensive margin, we vary the length of the POST period over two scenarios. In scenario A we use the same POST period we have been using up until this point in the analysis, which includes all transactions in January and February 2014. In scenario B we instead use transactions in March and April 2014 in the POST period. Figure 1.2a plots the β_1 coefficients from these regressions for the National Chain across the two time scenarios. Note that the effect of the DCB policy on paper bag consumption remains relatively stable over time, however, when using the later months the increase in the probability of bringing reusable bags is roughly 20 percentage points lower and the increase in the probability of using no bag is 20 percentage points higher. The differences in point estimates between the early and later scenarios are statistically significant at the 5% level for all bag types except paper. One hypothesis for why these changes occur is that customers learn they can repack their cart item by item without bags and then bring the cart out to their vehicle (where they may or may not have bags) to unload it again.[28]

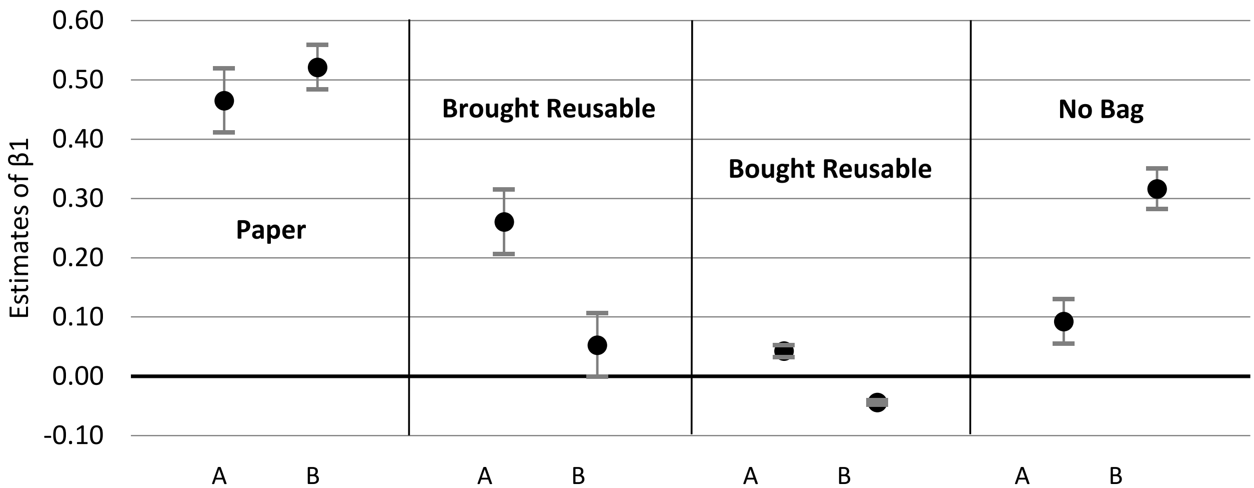
Figure 1.2b repeats the analysis above for the Discount Chain. Comparing the panels reiterates the point that the treatment leads to dissimilar bag usage patterns at the two grocery chains. Unlike National Chain shoppers, the increase in the probability of Discount

[27]Baggers are instructed to keep busy and float to checkout lanes where there are groceries to bag. Thus the correlation between bag use and baggers may be due to baggers moving to larger transactions that need bags.

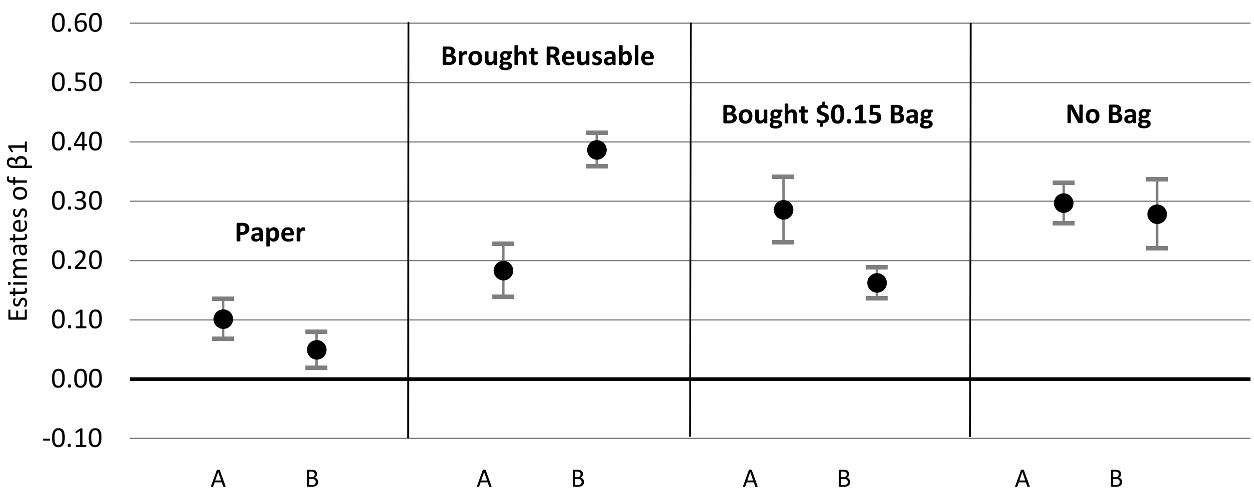
[28]Many interviewed managers noted that shopping cart and shopping basket theft increased after the policy change.

Figure 1.2: Effect of Plastic Bag Ban on bag Demand Over time

(a) **National Chain**



(b) **Discount Chain**



Note: 95% confidence intervals are calculated using clustered standard errors; β_1 is the coefficient on the interaction of Richmond*POST. Using the model specifications for bag demand on the extensive margin, we vary POST over two scenarios: (a) POST = January–February, and (b) POST = March–April.

Chain shoppers bringing a reusable bag is 20 percentage points higher in the later months. Also, the increase in the probability of buying a 15-cent reusable bag is 13 percentage points lower in the later months. The differences in point estimates between the scenarios are only significant at the 5% level for brought bags and bought 15-cent bags. We hypothesize that these changes occur in part because a fraction of customers begin reusing the 15-cent bags they purchased previously. We will discuss the reuse of the 15-cent bags further in the next section on policy implications.

1.5 Bag Bans vs. Bag Fees and Policy Implications

If the objective of DCB policies is to increase the use of reusable bags and decrease the use of disposable carryout bags, how do bag bans and bag fees compare in reaching this objective? To answer this question we compare the proportion of customers using reusable bags and disposable carryout bags in Homonoff's (2016) sample to the proportion of customers using reusable and disposable carryout bags in our sample. As mentioned above, Homonoff studies the 2012 Montgomery County Maryland DCB policy, which imposed a 5-cent fee on both disposable plastic and paper bags. In Homonoff's study, stores in Virginia were without a DCB policy and thus are comparable to our Concord stores. Stores in Washington DC had a five-cent bag fee for two years when the Montgomery County Maryland bag fee went into effect, and thus are comparable to the Berkeley stores in our sample. Lastly, the Maryland stores are the ones treated in Homonoff's study and thus are comparable to the Richmond stores in our sample. Note that while Maryland and Richmond customers face very different prices for thin plastic bags (five cents versus infinity), both face the same magnitude of fee for paper bags (five cents).

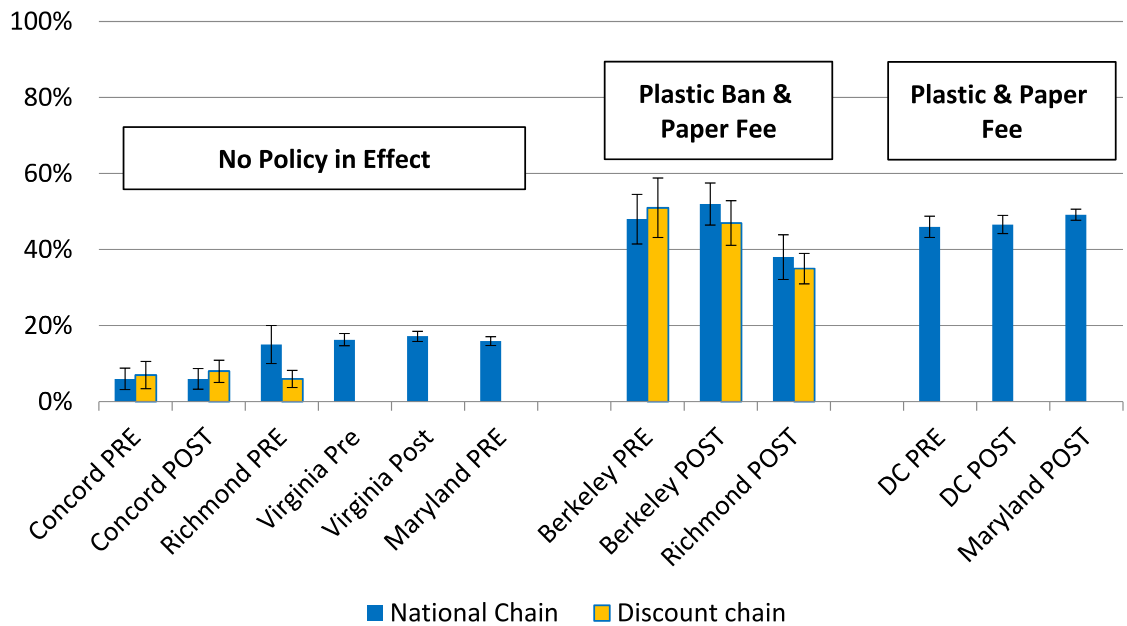
We first focus on the National Chain stores in our study since they are more comparable to the grocery chains used by Homonoff (2016).[29] The darkly-shaded bars in figure 1.3a plot the proportion of customers bringing reusable bags, and in figure 1.3b they plot the proportion of customers using disposable carryout bags (either thin plastic or paper) at National Chain stores across policy jurisdictions and treatment periods. In figure 1.3a we see that bag bans and fees have very similar effects on encouraging customers to bring reusable bags at National Chain stores—with 46% of customers facing a ban and 47% of customers facing a fee choosing to bring reusable bags. In figure 1.3b disposable carryout bag usage is also similar under both types of policies, though on average the proportion of bag ban customers using disposable carryout bags is 6 percentage points less than the proportion of bag fee customers. In summary, at National Chain stores bans and fees have similar effects on reusable bag usage, while bans lead to slightly less disposable carryout bag usage than fees. Yet what is not shown here is that disposable carryout bag use for ban customers is completely comprised of paper, whereas for fee customers it is a mix of paper and thin plastic.[30] Since paper bags have their own set of environmental costs, policymakers

[29]We also drop all January observations so that our POST period is more similar to that used by Homonoff (2016), whose data in the pre-period were collected from late September to early November of 2011, while data in the post-period were collected from late February to early March of 2012.

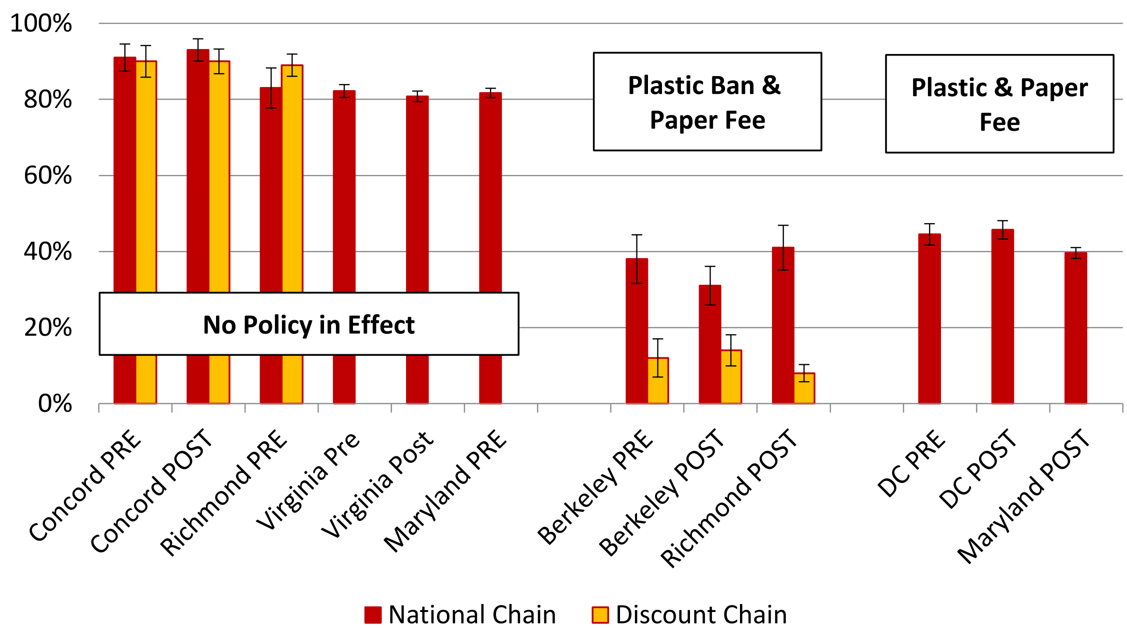
[30]Homonoff (2016) does not decompose disposable carryout bag usage into paper and plastic because "almost all customers chose to use plastic bags when they were offered." Looking at our pre-period data, we also find that the vast majority of customers chose plastic bags over paper bags when given the option.

Figure 1.3: Proportion of Customers Across Policy Jurisdictions (a) Bringing Reusable Bags, and (b) Using Disposable Bags

(a) **National Chain**



(b) **Discount Chain**



Note: Confidence intervals are at a 95% confidence level.

and citizens may be concerned by the significant increase in paper bag usage under plastic bag bans.

However, paper is not the only alternative type of bag that grocery stores can offer. The 15-cent thick-plastic bags are an alternative to thin plastic bags and they do not have as large side-effects of being easily blown around or clogging recycling equipment. This begs the question: How does the proportion of customers using reusable and disposable bags under a bag ban with the 15-cent bag option compare to bag usage under policies without this alternative? To answer this question, we compare bag usage at our Discount Chain stores to bag usage at our National Chain stores and stores in Homonoff's sample.[31] Focusing on the lighter-shaded bars, in figure 1.3a, we see that having an alternative type of bag to buy does not significantly alter the proportion of customers bringing reusable bags. In other words, reusable bag usage is stable across DCB policies and store types. However, in figure 1.3b, we find that the presence of an alternative bag to purchase has a large effect on the proportion of customers using disposable carryout bags. Only 10% of customers at Discount Chain stores with bans use disposable paper bags, compared to roughly 40% of customers at stores without 15-cent thick-plastic bags. Therefore, 15-cent thick-plastic bags seem like desirable alternatives to thin plastic bags, especially for those who worry that bag bans lead to an increase in paper bag consumption. In light of concerns that the thick-plastic bags are not actually reused by customers, our unique experimental design allows us not only to measure the number of customers buying the thick-plastic bags, but also the number reusing them as well. At Discount Chain stores with a bag ban in place, 39% of customers brought at least one reusable bag. Of those customers bringing reusable bags, 17% brought the 15-cent reusable bag. In comparison, only 6% of customers bringing reusable bags brought old paper bags. Moreover, comparing the percentages of customers buying and bringing 15-cent bags indicates that two out of five customers that buy 15-cent bags reuse them in future purchases. Therefore, a large percentage of Discount Chain customers do reuse the 15-cent thick-plastic bags for future purchases. And this is just a lower bound for the reuse of these bags, which may be reused in other ways by customers (e.g., bag liners for small garbage bins).

Given these results, our first policy recommendation for municipalities considering DCB policies is that retailers should be required to make the price of all types of bag they offer transparent to their customers at purchase. Just by having a salient price, much of the externality of the overuse of disposable carryout bags is eliminated. Second, for cities, counties, and states that opt for plastic bag bans with paper bag fees, we recommend incentivizing the production and sale of 15-cent thick-plastic reusable bags. The thickness of these bags makes them less likely to blow out of garbage systems and into waterways. Plus, they are both recyclable and reusable. Furthermore, we find that when Discount Chain stores offer their price-sensitive customers 15-cent bags, it produces similar levels of reusable bag usage and smaller levels of paper bag usage than at National Chain stores without these bags.

[31] When making this comparison, we want to keep in mind the caveat that the clientele at Discount Chain stores is very different than at National Chain stores.

1.6 Conclusions

This paper adds empirical insight to the "prices vs. quantities" debate by being the first to compare the effect of bans versus fees on consumers' disposable and reusable bag demand. We collect observational bag usage data (over bag type, time, and space) to measure shifts in bag demand in response to the introduction of a plastic bag ban coupled with a paper bag fee. We then compare our analysis to a study on a plastic and paper bag fee. Our main results are threefold. First, bag bans—while decreasing total disposable carryout bag consumption and increasing reusable bag usage—lead to significant increases in paper bag demand. At the National Chain, the percentage of customers using paper bags climbs from less than 5% before the policy to over 40% afterwards. However, the fee for paper bags is effective in that those who choose to buy paper use fewer paper bags per transaction after the policy change. Second, we find that the two DCB policies produce remarkably similar increases in reusable bag usage. Third, bans and fees also have similar effects in terms of reducing total disposable carryout bag consumption, that is, unless stores offer inexpensive reusable bags and charge more for paper bags, in which case bans may be more effective than fees. In particular, the percentage of customers using paper only increases to 10% at Discount Chain stores where 15-cent reusable bags were sold. Thus, if the environmental costs of both plastic and paper bag consumption are a concern to policymakers and the public, these results suggest that DCB policies should also regulate the types and prices of the reusable bags that stores choose to sell instead of—or alongside—disposable carryout bags.

Our results are not only relevant for the regulation of disposable carryout bags, but can also inform policies regulating other disposable products, such as Styrofoam containers and plastic bottles. Future work will focus on other unintended effects of DCB policies. We aim to merge our observational bag usage data with corresponding transaction-level scanner data to measure whether bag policies lead to changes in the composition of shopping baskets and in the time necessary for checkout. While bans and fees were found to have similar effects on reusable bag usage in this study, these policies may differ along other dimensions.

Chapter 2

Waiting in Line: The Hidden Time Costs of Changing Behavior

2.1 Introduction

Governments often enact policies to incentivize consumers away from behaviors with negative externalities, at the expense of consumer convenience. For example, policies to combat airborne pollutants and congestion from driving—such as driving restrictions by license plate number (Davis, 2008), high-occupancy vehicle lanes (Kwon and Varaiya, 2008), and time-varying road pricing (Gibson and Carnovale, 2015)—incentivize consumers to spend time and effort in altering when and how they drive their vehicles. Energy efficiency subsidies (Allcott, 2016; Fowlie et al., 2015), garbage pricing (Fullerton and Kinnaman, 1996), and bottle return refunds (Beatty et al., 2007; Ashenmiller, 2011) encourage consumers to spend time and effort in conserving energy, reducing waste, and recycling. These policies illustrate that when the environment and consumer convenience are at odds with one another, policymakers "ask" consumers to trade convenience to benefit the environment. This begs the empirical question: what are the time, effort, and psychological costs consumers trade in changing their behavior? While the economic literature widely acknowledges the importance of non-monetary costs—for (i) improving policy design, (ii) conducting welfare analysis, and (iii) avoiding unintended consequences[1]—quantifying these costs is often infeasible, causing

[1]The importance of non-monetary costs in changing consumer behavior is acknowledged for three key reasons. First, policies often have greater success in changing behavior when the trade-offs consumers face are incorporated into the policy design. In the behavioral economics literature, numerous studies have shown how option defaults can be set recognizing the non-monetary costs of opting out, such as in the case of retirement savings (Madrian and Shea, 2001; Choi et al., 2003) and organ donation (Johnson and Goldstein, 2003). In the technology adoption literature, a frequent conclusion is that consumers may not adopt a privately beneficial product, even when it is free, if the non-monetary costs of obtaining or using the product are high (Dupas, 2014; Fowlie et al., 2015). Second, not understanding the non-monetary costs of a policy can lead to unintended and perverse consequences, if consumers try to avoid inconvenient policies with riskier or more harmful behavior. For example, with respect to the unintended consequence of driving restrictions

them to be easily overlooked. This is particularly true when the population is vast and heterogeneous and the behaviors to be altered are frequent but individually short-lived.

In this paper, I explore a hidden time cost of an environmental policy aimed at altering consumer behavior. Specifically, I examine how local government regulation of disposable carryout bags (DCB) affects the wait and processing time of checkout services provided by supermarkets. While DCBs bring convenience to supermarkets and supermarket customers, they are costly to the environment and to governments trying to keep their streets and waterways clean. In order to curb the consumption of single-use bags and encourage the use of reusable bags, DCB policies prohibit retail stores from providing customers with "free" bags at checkout.[2] Using high-frequency scanner data from a national supermarket chain and variation in DCB policy adoption over time and space in an event study empirical strategy, this paper addresses two fundamental questions: 1) What are the time costs to individual consumers of policy-induced behavioral changes, and 2) How do these costs evolve as people learn and adapt their behavior?

Several features of supermarket checkout and DCB policies make them an interesting setting to study the time costs of changing behavior. First, food shopping is a common, frequent, and arguably necessary behavior. In the United States, consumers purchase the majority of food at grocery stores, supermarkets, and superstores (Taylor and Villas-Boas, 2016a), with the average American adult grocery shopping once every 7.2 days and spending 44 minutes in-store per trip (Hamrick et al., 2011). This aggregates to 11.9 billion grocery shopping trips and 8.7 billion hours in-store each year in the United States.[3]

Second, supermarket checkout is a setting where changes in the time spent in an activity can be directly, and precisely, identified and measured. With time-stamped, transaction-level scanner data obtained from a national supermarket, I know exactly when and where a checkout transaction occurred (e.g., Register 2 in Store X and City Y on Saturday, April 27, 2013 at 2:07pm), who was present (e.g., Cashier A and Customer B), what was purchased (e.g., two boxes of Crispy Crunch Cereal at $3.79 each), and importantly, how much time the transaction took to complete. Unlike in previous studies, these data do not rely on surveys and time diaries, which can be expensive to implement and are prone to systematic under/over reporting and recall bias (Neter and Waksberg, 1964; Mathiowetz and Duncan, 1988). Moreover, the panel nature of the scanner data allows me to examine the effects of the policy change over time, at the store, cashier, and customer level.

Third, DCB policies are widely used legislative tools for changing consumer behavior. With DCB clean-up, recycling, and landfilling costing local governments millions of dollars per year,[4] lawmakers across the country have adopted DCB policies to change how their

by license plate number, Davis (2008) found that consumers circumvented the policy by increasing the total number of vehicles in circulation. Third, accurate welfare analysis requires a complete picture of a policy's costs, including the non-monetary costs paid by individual consumers (Allcott and Kessler, 2015).

[2]Retail stores pass the cost of disposable bags on to their customers in the overall price of groceries.

[3]Author's calculation using population data from the 2010 United States Census.

[4]Local governments are estimated to spend 1.1 cents per bag in collection, processing, and landfilling costs (Herrera Environmental Consultants, Inc., 2008). Given that approximately 100 billion plastic bags are

constituents obtain food. From 2007 through 2016, approximately 242 local government DCB policies were adopted across 20 states and the District of Columbia.[5]

Fourth, while lawmakers acknowledge the trade-off between convenience and the environment in regulating bags,[6] these policies have been typically evaluated based on the magnitude of behavior change and litter reduction, and not full social welfare. Several studies have found DCB policies to be quite effective in altering consumer bag choices (Taylor and Villas-Boas, 2016b; Homonoff, 2016; Convery et al., 2007; Dikgang et al., 2012).[7] However, little is known about how these policy-induced behavioral changes affect the time and convenience of individual consumers. Given the extensive literature showing that shopping convenience impacts where and what people purchase to eat,[8] and the literature showing that consumers dislike and actively avoid long wait times (Katz et al., 1991; Tom and Lucey, 1995; Hirogaki, 2014),[9] it is important to understand the trade-offs between convenient behaviors and environmentally-friendly behaviors in food acquisition.

I hypothesize that DCB policies increase the duration of checkout through each of the three main inputs into the production function of supermarket checkout—namely, (i) bags, (ii) labor, and (iii) capital. First, DCB policies directly change the choice set of bags and their prices, and different bags vary in packing time. Second, to implement DCB policies, cashiers must learn new key codes and procedures for collecting fees. Cashiers and baggers must also ascertain the number and types of bags customers want, and how to pack them. If customers do not bring bags, customers must decide how many bags for which they are willing to pay. This turns a decision that was automatic and habitual (i.e., fast thinking) into an economic, utility maximizing decision (i.e., slow thinking) (Kahneman, 2011).[10] Third, checkout lanes

consumed in the U.S. each year (Clapp and Swanston, 2009), municipalities nationwide spend $1.1 billion per year to manage plastic bags.

[5]Numerous major U.S. cities have adopted DCB policies, including San Francisco, Washington DC, San Jose, Seattle, Austin, Boulder, Los Angeles, Sante Fe, Chicago, Minneapolis, and New York City. For a list of DCB policies by city, county, and state, see: *Californians Against Waste.* Online, *accessed Sep. 5, 2016.*

[6]For instance, the City of Portland states, "Single-use plastic carryout bags may offer short-term convenience, but they have long-term costs. Not only do single-use bags require resources such as petroleum and natural gas to manufacture, their disposal presents a number of problems as well." (Online, *accessed Sep. 10, 2016*).

[7]Taylor and Villas-Boas (2016b) finds that a plastic bag ban coupled with a paper bag fee in California led to a 26 percentage point (ppt) increase in the use of reusable bags and a 9ppt increase in the use of no bags. However, the eradication of plastic bags was offset by a 47ppt increase in the use of paper bags. Homonoff (2016) studies the impact of a plastic and paper bag tax that went into effect in Montgomery County, Maryland and finds that the share of transactions using disposable plastic bags declined by 42ppt after the tax implementation. Additional studies have found DCB policies to be effective in changing bag choice in Ireland (Convery et al., 2007) and South Africa (Dikgang et al., 2012).

[8]See Yaktine and Caswell (2013) for a comprehensive review of this literature.

[9]Moreover, not only do long lines have a time cost, they also have an emotional one: "stress, boredom, that nagging sensation that one's life is slipping away."("Why Waiting is Torture." *New York Times.* Aug. 19, 2012. Online, *accessed Mar. 25, 2016.*)

[10]Kahneman's (2011) proposes a the dichotomy between two modes of thought—"System 1" is fast, instinctive, and subconscious and "System 2" is slower, more deliberative, and more conscious. With economic incentive and regulations, policymakers are forcing people to switch from fast thinking habits (System 1) to

are optimized for single-use plastic bags, and the number of checkout lanes is optimized to handle checkout traffic during peak shopping hours. Importantly, checkout machinery is *fixed in the short-run*. During non-peak hours, if transactions are slower, retailers have the option to open more lanes to ease congestion at the cost of paying additional cashiers. However, during peak hours, retailers are constrained by their fixed checkout capital, and thus, slower transactions translate to increased checkout congestion and longer wait times for customers. Therefore, there exist several mechanisms through which DCB policies could lead to longer checkout wait and processing time, some of which may be reduced over time through learning-by-doing and learning-by-using (Arrow, 1962; Rosenberg, 1982). My analyses will shed light into each of these mechanisms.

To identify the time cost of DCB policies on checkout duration, I exploit a quasi-experiment in California, where city and county DCB policy adoption has varied across both time and space. Leveraging this spatial and temporal variation to control for potentially confounding factors, I employ an event study empirical strategy. The event study model identifies the time cost of DCB policies on checkout duration (my first research question) by comparing checkout duration at stores in jurisdictions with DCB policies to checkout duration at stores in jurisdictions yet to be treated and in jurisdictions that are not treated during the sample. Importantly, plotting the differences between treated and control supermarkets over event-time enables me to directly test the identifying assumption of parallel trends in the pre-policy period, and to explore the dynamics of the policy effects in the post-policy period (my second research question). For the event study analysis, I design a subset of scanner data, selecting data from comparable treated and control stores across California between January 2011 and May 2014. In total, the dataset contains 9.8 million checkout transactions made during 1,047 peak shopping hours across 53 supermarkets.

My event study results reveal that DCB policies cause a 3% average increase in checkout transaction duration. I document heterogeneity in the policy effects by transaction size (i.e., the number of items purchased) and by whether a customer chooses to pay for paper bags at checkout, with the smallest transactions not paying the disposable bag fee experiencing no slowdown and the largest transactions paying for disposable bags experiencing a 10% slowdown. Surprisingly, even though I observe evidence of learning at the cashier level, this learning does not eliminate the slowdown from DCB policies, which persists over the entire sample period.

While 3% slower checkout durations (or roughly 3.6 seconds more per customer) may seem negligible, over the 11.9 billion grocery shopping trips made per year in the U.S., this time cost aggregates quickly. Moreover, shoppers experience both the slowdown of their own transaction and the slowdown of all transactions ahead of them in line. I find that DCB policies lead to a significant increase in checkout congestion during peak shopping hours, with 18 fewer customers processed per store per three-hour shift. Using a simple queuing theory model, 18 fewer transactions means each checkout queue is 1 customer longer on average.

slow thinking optimization (System 2). As people adapt to the policies, they may return to the speed and ease of System 1.

Using supplementary data, I provide suggestive evidence that the transactions lost during peak hours shift into the previously less busy shoulder hours, where stores have the capacity to open more lanes. Aggregating to the state level, the longer wait and processing time from DCB policies would cost Californians 25.8 million hours annually (≈$343 million). In comparison, the collection, processing, and landfilling of DCBs is estimated to cost Californian taxpayers $154 million per year.[11] This taxpayer estimate does not include the environmental cost of plastic marine debris. Therefore, while the aggregate time cost I estimate exceeds the amount currently paid by Californians in managing plastic bags, it might not exceed the long run environmental costs of plastic in oceans and waterways.

I conduct a series of robustness checks to further explore the results and their external validity. First, I test the robustness of the scanner data results to the use of an alternative data source—observational data collected in-store before and after a DCB policy change. I estimate results consistent with the scanner data, demonstrating that missing variables in the scanner data (i.e., the presence of baggers, the types of bags purchased, and the gender and race of the customer) are not biasing my results. Second, I replicate the analysis on supplementary data from an alternative store chain—a regional discount chain targeting low-income and bargain shoppers. I show the effects of DCB policies are not unique to the main retail chain in this paper. Third, I replicate the analysis using the 2010 Washington DC bag tax—a policy in a different location, with a different regulation tool. With scanner data from stores in the DC metropolitan area, I again observe checkout slowdowns due to the policy change; however, unlike the California bag bans, the slowdown from the bag tax lessens significantly over time.

This paper contributes to an emerging literature on the hidden costs of changing consumer behavior.[12] To my knowledge, I am the first (i) to quantify the time cost of a policy change separately from other non-monetary costs, (ii) to examine how this recurring cost evolves as behaviors and habits adjust to the policy, and (iii) to focus on a policy and setting where capacity constraints determine whether retailers or customers bear the incidence of the time cost. My results also relate to the literature on congestion and waiting—which concludes that people place a higher value on time spent waiting than they do on the same amount of time in other circumstances (Maister, 1985; Larson, 1987; Small and Verhoef, 2007; Abrantes and Wardman, 2011)—and has implications for policies where governments intervene to protect citizens from their own choices. Economic incentives and regulations which seem like low-cost behavioral nudges, may have large non-monetary costs with respect to time and convenience

[11] Author's calculations, given that Californians are estimated to consume 14 billion plastic bags per year (CA Senate Rules Committee, 2014) and that DCB collection, processing, and landfilling is estimated to cost taxpayers 1.1 cents per bag (Herrera Environmental Consultants, Inc., 2008).

[12] Just and Hanks (2015) model the hidden emotional costs of command-and-control policies and argue that ignoring emotional responses to policy change may cause significant deadweight loss. Allcott and Kessler (2015) evaluate the welfare effects of social comparisons in reducing energy consumption and show that ignoring the time, comfort, and psychological costs of the intervention overstates the welfare gain of the program by a factor of five. In a similar vein, Damgaard and Gravert (2016) study the annoyance cost of a nudge intervention and show that when not accounting for the hidden costs of reminders, the average welfare effects are overstated by a factor of ten.

when aggregated across all consumers and all consumption occasions, especially in settings where consumer behaviors are connected through queuing systems. While often challenging to measure, and thus easy to overlook, quantifying these costs is vital for accurate welfare analysis and improved policy design.

The remainder of the paper is organized as follows. Section 2.2 describes the setting, empirical design, and data. Section 2.3 describes the event study regression model. Section 2.4 presents the main results. Section 2.5 rules out alternative mechanisms behind the transaction slowdown. Section 2.6 uses three supplementary datasets to explore the external validity of the results and perform robustness checks. Section 2.7 discusses the broader impacts of the time costs of DCB policies. Section 2.8 concludes.

2.2 Setting, Research Design, and Data

Background on Disposable Carryout Bags and Regulations

When first invented, plastic carryout bags were considered quite the engineering feat: "a waterproof, durable, featherweight packet capable of holding more than a thousand times its weight" (Freinkel, 2011). However, the characteristics that make plastic bags convenient also make them costly to the environment and to municipalities trying to keep their streets and waterways clean. Their lightweight and aerodynamics make it easy for them to blow out of waste streams and into the environment and waterways, where, due to their durability and water-resistance, they last for a long time. While the majority of single-use plastic bags are landfilled or littered, even when properly recycled, they can clog the machinery used to sort materials.

Each year Americans consume approximately 100 billion single-use plastic bags (Clapp and Swanston, 2009)—over 300 bags per person per year. Local governments are estimated to spend 1.1 cents per bag in clean-up, processing, and landfilling (Herrera Environmental Consultants, Inc., 2008), which aggregates to municipalities nationwide spending $1.1 billion per year. This clean-up cost estimate does not include the environmental costs of plastic marine debris. Jambeck et al. (2015) calculate that 1.7-4.6% of the plastic waste generated in coastal countries around the globe is mismanaged and enters the ocean. Once in waterways, plastic bags do not biodegrade, but instead break into smaller pieces, which can be consumed by fish, turtles, and whales that mistake them for food.[13]

Given the environmental and clean-up costs of DCBs, lawmakers across the country are turning to policies to regulate DCBs. As of December 2016, approximately 242 local

[13] A survey of experts, representing 19 fields of study, rank plastic bags and plastic utensils as the fourth severest threat to sea turtles, birds, and marine animals in terms of entanglement, ingestions, and contamination (Wilcox et al., 2016). While plastic bags and films represent only 2.2% of the total waste stream (CA Senate Rules Committee, 2014), plastic grocery bags and other plastic bags are the eighth and sixth most common item found in coastal cleanups ("International Coastal Cleanup. Annual Report 2016." *Ocean Conservancy.* Online, *accessed Jul. 26, 2016*).

government DCB policies had been adopted across 20 states and the District of Columbia.[14] DCB policies prohibit retail stores from providing customers with "free" bags at checkout, with the goal of curbing the consumption of single-use bags and encouraging the use of reusable bags. These policies use one or both of the following policy tools to alter consumer behavior: (1) bag bans—command-and-control approaches to regulate behavior directly (i.e., quantity regulations), and (2) bag fees—market-based approaches to incentive consumers to change their own behavior (i.e., price regulations).

California provides a rare quasi-experiment for analyzing the effects of DCB policies on checkout duration and learning. In California, DCB policies ban retail food stores from providing customers with disposable plastic carryout bags under 2.25 mils thick (i.e., traditional plastic carryout bags) and require stores to charge a minimum fee for all paper and reusable carryout bags provided at checkout.[15,16] From 2007 through 2014, 82 DCB policies were implemented in California, covering 111 city and county jurisdictions and roughly one third of California's population.[17] This local legislative momentum culminated with the nation's first statewide plastic bag ban, which was voted into law on November 8, 2016.[18]

Figure 2.1 maps the implementation of DCB policies at four points in time. City-level policies are depicted with dark green circles. Unincorporated county policies are shaded in light yellow. Countywide policies—where all unincorporated areas and all cities in a county implement DCB regulations—are shaded in dark green.[19] This figure highlights the fact that DCB policies have varied greatly across both implementation dates and locations. My event study empirical strategy exploits the variation in DCB policies across time and space from this quasi-experiment to explore how DCB policies influence checkout duration and learning.

[14]For lists of disposable bag policies by city, county, and state (and by adoption/rejection date), see: BagLaw.com and Californians Against Waste, *accessed Sep. 5, 2016*.

[15]While the vast majority of DCB policies in California require a 10-cent fee for paper and reusable bags, a handful of jurisdictions have opted for either no fee, a 5-cent fee, or a 25-cent fee.

[16]Why has California chosen bans over fees? California Assembly Bill 2449, enacted in 2006, began as a plastic bag fee bill, but due to pressure from the plastic industry, transformed into a plastic bag recycling bill. Additionally, this bill temporarily prohibited any public agency from adopting a regulation that imposed a plastic bag fee upon a store. Consequently, a bag fee was not an available policy tool for local governments in California that wanted to regulate plastic bags. ("The Plastic Bag Ban Epic." *LA Observed.* Sep. 6, 2014. Online, *accessed Oct. 9, 2016*).

[17] Author's calculations. See Appendix Table A.1 for a list of California DCB policies and implementation dates from 2007 to 2014.

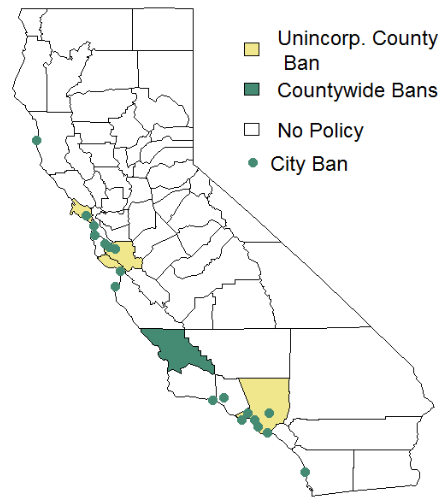
[18]While a statewide ban on plastic bags passed the California state legislature and was signed into law by the governor on September 30, 2014, opponents secured enough signatures to put the ban to a public referendum. On November 8, 2016, Californians voted and passed the Plastic Bag Ban Referendum (Proposition 67) by a margin of 52.9% (yes) to 47.1% (no).

[19]Similar to other local government waste regulations, DCB policies may be implemented by city councils (for incorporated areas), county boards of supervisors (for unincorporated areas), and county waste management authorities (for entire counties with opt-out options for incorporated areas).

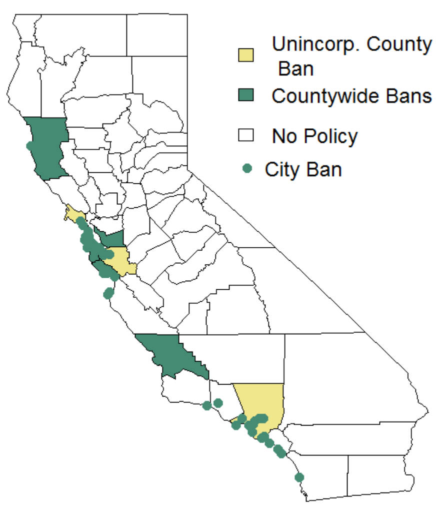
Figure 2.1: California Disposable Carryout Bag (DCB) Policies over Time

(a) Policies Implemented Before 2012

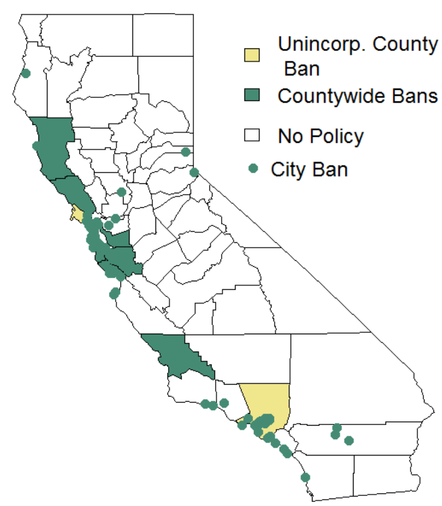
(b) Policies Implemented Before 2013



(c) Policies Implemented Before 2014



(d) Policies Implemented Before 2015



Note: The local governments of unincorporated counties and incorporated cities can pass ordinances to regulate disposable carryout bags. Countywide policies occur when all cities and unincorporated areas in a county pass DCB regulations.

Sample Selection for Scanner Data

Quantifying the shock to checkout duration from DCB policies requires a detailed dataset on the speed and location of checkout transactions. To this end, I obtained access to time-stamped scanner data from a national supermarket chain.[20] While this retail chain processes as many as 800,000 items per hour in California alone, there was a limit to the amount of data I could request at the transaction level. Thus I designed a subset of scanner data, selecting data from comparable treated and control stores across California between January 2011 and May 2014.

My procedure for selecting the stores was as follows. First, the retailer provided a list of their stores in California with basic characteristics, such as street address, city, zip code, date opened, last date remodeled, and building area size. I merged in store level demographic data, created by Gicheva et al. (2010) using 2000 US Census data for each store's census block-group. Next, I split the sample of stores into treated and control, using the database of DCB policies I constructed for California.[17] As a first step in ensuring that control stores are good counterfactuals for treated stores, I dropped all stores in counties where no DCB policies had been adopted yet. As a second step, I used a propensity score matching algorithm, based on store age, store size, and stores' census block-group characteristics, to select 30 pairs of treated and control stores with sufficient overlap in observables. The data request for these stores was submitted in July 2013. By the time the request was approved and the data were pulled in May 2014, additional DCB policies had been enacted, affecting 8 of the control stores. Furthermore, after receiving the data I decided to drop 7 stores which experienced either closure, remodeling, or policy differences, as these events could confound my checkout productivity measures.[21] Thus, in the final sample I have 53 stores—33 treated and 20 control—across 45 policy jurisdictions (i.e., 45 incorporated city and unincorporated county jurisdictions).

Importantly, the treated stores were chosen to mirror the variation of policy implementation dates in California. Figure 2.2 presents the number of municipalities in California implementing a DCB policies (depicted by the gray bars) and the number of stores in my sample in jurisdictions implementing a DCB policy (depicted by the black bars) in each month over the sample period—which spans January 2011 through May 2014. As designed, the distribution of implementation dates for stores in my sample roughly matches the distribution of policy implementation dates across California.[22] Also, none of the stores are in

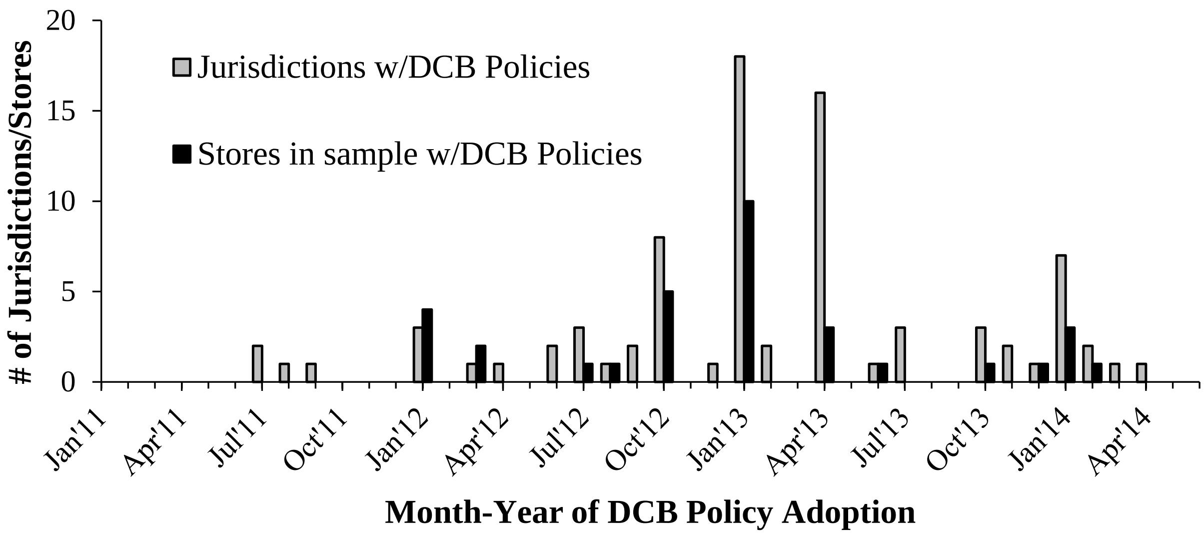
[20] There are over 2000 locations of this supermarket chain across the U.S. With revenue over $35 billion per year, this chain is one of the 15 largest retailers in the U.S.

[21] Of the 7 dropped stores, 2 store closed before the end of the sample period, 4 stores were remodeled to add self-checkout registers, and 1 store was in a jurisdiction where the DCB policy differed from the others in the sample in that it did not require a fee for paper bags. Given 5 of the dropped stores were in the treated group and 2 were in the control group, I lose 13% of the treated stores and 9% of the control stores.

[22] Jurisdictions decide when DCB policies will be operative, not the stores in a jurisdiction. The operative date is specified in a jurisdiction's ordinance document (i.e., bill) which is passed and adopted into law. Examining the ordinance documents of all 111 jurisdictions in California that implement DCB policies between 2007 and 2014, I find that 21% of jurisdiction specified January 1 as the operative date, 30% specified

jurisdictions that implemented policies before 2012, which means I have a full year of 2011 data in the pre-period for all stores.

Figure 2.2: Number of California Jurisdictions Implementing DCB Policies by Month and Year



Source: Author's calculations.

The necessary identifying assumption for an event study design is that treated and control stores have parallel trends in the outcome variable pre-policy. Having stores that are also well matched on observables increases confidence that this assumption is satisfied. The top panel of Table 2.1 presents average store characteristics for treated and control stores. None of the variables are statistically different between treated and control groups. On average, stores in my sample first opened in 1985 and were remodeled in 2005. The majority of stores have bakery, deli, and floral departments, a little over half of the stores have pharmacies and coffee bars, and 10% or less have gas stations, juice bars, and sandwich counters. Roughly 45% of the stores (both treated and control) have self-checkout registers.[23] The bottom panel of Table 2.1 presents the summary statistics for average store demographics across treatment

the first of a month that was not January, 14% chose Earth Day (April 22), 11% chose a specific date other than the first of the month, and 23% did not specify a specific date and instead wrote to be operative 1, 3, or 6 months after adoption. Implementation dates vary across all days of the week. Importantly, while operative dates were not randomly chosen, the dates were also not selected in a systematic way across all jurisdictions which would bias the results.

[23]Stores with self-checkout are systematically larger in square footage than those without. When the stores are ranked by size, all 13 stores greater than 53,000 ft^2 have self-checkout, while all 24 stores less than 41,500 ft^2 do not. To further understand how stores with and without self-checkout registers differ, I replicate Table 2.1 by self-checkout status. While stores without self-checkout are 10 years older and less

Table 2.1: Average Store Characteristics and Demographics

	(1) Control Stores	(2) Treat Stores	(3) P-value of Diff.	(4) California	(5) United States
Store Characteristics					
Building Size (ft^2)	41,782.75	43,434.45	0.632		
Open Year	1985	1984	0.778		
Last Remodel Year	2005	2005	0.992		
Departments & Services (share)					
Bakery	0.75	0.79	0.755		
Pharmacy	0.50	0.64	0.338		
Deli	1.00	0.94	0.270		
Floral	0.90	0.91	0.915		
Coffee Bar	0.70	0.52	0.193		
Gas Station	0.05	0.06	0.874		
Juice Bar	0.10	0.09	0.915		
Sandwich Counter	0.05	0.06	0.874		
Self-checkout registers (share)	0.45	0.48	0.810		
Store Location Demographics					
Median Income ($)	$63,120	$62,280	0.874	$47,493	$41,994
Household Size (#)	2.61	2.58	0.726	2.87	2.59
White (share)	0.70	0.69	0.785	0.60	0.75
Black (share)	0.06	0.05	0.723	0.07	0.12
Asian (share)	0.11	0.12	0.590	0.11	0.04
Over 65 (share)	0.12	0.12	0.956	0.11	0.12
Do not own vehicle (share)	0.06	0.06	0.415	0.10	0.10
Urban (share)	0.75	0.82	0.562	0.87	0.79
N Stores	20	33			

Note: Asterisks indicate the following: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Store characteristic data were provided by the retailer. Store demographic data come from Gicheva et al. (2010), who use 2000 US Census data for each store's census block-group. The state and country level data come from the 2000 US Census, *Table DP-1. Profile of General Demographic Characteristics: 2000*; Geographic Areas: California and United States. Urban areas are locations with populations densities greater than 500 people per square mile.

groups.[24] Once again, none of the variables are statistically different across treatment groups. Table 2.1 also presents the average demographics for California and for the United States. Comparing columns (1) and (2) with column (4), the stores in my sample are in areas with higher median incomes and a greater share of White residents than the California averages. These differences reflect that fact that DCB policy adoption occurred first in coastal California regions, which are more affluent on average than the Central Valley regions.

Finally, due to the constraints in obtaining data from the retailer, the sample includes only the hours between 1:00pm and 4:00pm for every Saturday and Sunday during the sample years. I chose these weekend afternoon hours because the retailer cited them as peak shopping hours in their stores.[25] Having peak hours assures that transactions in the scanner data occur back-to-back, with little or no downtime in between. During these hours, the dataset includes every individual item purchased or returned at each store. In total, I have 1,047 hours of data across 53 stores, for approximately 133 million items scanned and 9.8 million transactions.[26]

Outcome Variables

Each observation in the scanner dataset corresponds to a purchased item, which I group into checkout transactions using a transaction identifier. For each item purchased within each transaction, the scanner data includes information on the item's name, Universal Product Code, and the purchase price. For each checkout transaction, the data include the time and date the transaction completed, the store identifier, the checkout lane number, a masked cashier identifier, and a masked customer card identifier. Using the identifiers, I am able to track stores, as well as cashiers and a subset of customers that frequently use rewards cards, over time.[27]

My main measure of checkout productivity for pre- and post-policy comparisons is: *Transaction Duration*—the duration of each checkout transaction measured in minutes, from the start of a transaction until the start of the next transaction in line. I am able to construct this variable using the transaction time-stamp, which includes the day, hour, and minute each transaction was completed. Since I only have one time-stamp per transaction, I designed

likely to have many of the departments that stores with self-checkout have (such as coffee and juice bars), stores with and without self-checkout are balanced across all demographic characteristics.

[24] As mentioned above, these variables were created by Gicheva et al. (2010) using 2000 US Census data for each store's census block-group.

[25] Before pulling the scanner data, I asked the retailer for their peak hours. I verified the hours they provided with Google store hour data. Importantly, while I find that 1:00-4:00pm on weekends are peak shopping hours, they are not the only peak hours in a week. Additional peak hours include 9:00am-5:00pm on weekends and 3:00-6:00pm on week days.

[26] I drop December 25 from the sample as not all stores are open on Christmas. I also drop Super Bowl Sundays as shopping patterns differ greatly on these days. Finally, I drop 142 transactions with more than 200 items scanned, as these were outliers.

[27] For the main analysis (Section 2.4), I use panel data averaged to the store level. In the sensitivity analyses (Section 2.5), I use panel data averaged to the cashier and customer level.

the sample to include only peak hours partly in order to make the assumption that transactions occur back-to-back, with little or no downtime in between.[28,29] My second measure of checkout productivity is: *Transactions per Shift*—the number of transactions completed in a store per 1:00-4:00pm weekend shift.

Table 2.2 presents transaction-level summary statistics for 2011, which predate all DCB policies in my sample. Transactions are separated by the register type in which they occurred—1) full-service, 2) express, and 3) self-checkout.[30] Overall, Table 2.2 indicates that treated and control stores have balanced transaction-level characteristics in the pre-period. At treated stores, the average transaction at a full-service register takes 1.99 minutes to complete, comprises of 19.22 items, and costs $56.38.[31] The average transaction at an express register takes 1.48 minutes to complete, comprises of 8.57 items, and costs $25.89.[32] Finally, the average transaction at self-checkout registers takes longer to complete, contains fewer items, and costs less than at either full-service or express registers.

To better understand checkout productivity at the store level, Table 2.3 reports average store-shift characteristics in 2011 for treated and control stores. In the pre-policy period, treated stores process approximately 567 transactions, $21,811 in sales, and 7,380 items per 1:00-4:00pm weekend shift. While full-service registers process only 50% of these transactions, they process over 70% of the items scanned and money spent.

Table 2.3 also presents the number of registers open on average and the total register capacity. To calculate the average number of registers open, I count the number of registers reporting at least one transaction per hour interval during the 1:00-4:00pm shift. Comparing the average number of registers open to the stores' register capacity, I find that stores are operating close to their full register capacity, at 2 fewer registers open than capacity on average. This suggests that during the peak weekend hours of 1:00-4:00pm, stores may be constrained by their fixed checkout capital. This will limit how stores can react to increases in checkout duration and congestion due to a policy shock in the short run.

[28] In Section 2.5, I verify this assumption using observational data collected in-store at checkout.

[29] To ensure that transactions occur back-to-back, I drop all transactions that are more than three standard deviations longer than the average transaction of its size (in terms of number of items scanned) and all transactions that are longer than 20 minutes. In cleaning the data this way, I lose 1.71 percent of transactions.

[30] Express registers have prominent signs overhead requesting shoppers to limit transactions to 15 items or fewer. Full-service registers have no recommended item limit. Self-checkout registers are registers where shoppers scan and bag their own items. I do not include transactions at specialty registers (e.g., registers in customer service, deli, and bakery departments) because there are few of these transactions and they rarely occur back-to-back.

[31] The amount paid is created by summing up the individual amounts paid per item in a transaction. This variable does not include sales tax. Furthermore, several line items, including the line item for purchasing a paper bag and for making a donations to charity, do not include an amount paid.

[32] Transactions in express lanes are statistically different in treated and control stores, with express transactions in treated stores being larger in size and longer in duration than in control stores.

Table 2.2: Average Transaction-Level Characteristics in 2011

	Control	Treat	P-value of Diff.
Transaction Duration (minutes)	**1.97**	**2.08**	**0.445**
Full-Service	1.89	1.99	0.160
Express	1.38	1.48	0.033**
Self-Checkout	3.70	3.98	0.078*
Items Scanned (#)	**13.17**	**13.44**	**0.629**
Full-Service	18.65	19.22	0.658
Express	7.64	8.57	0.046**
Self-Checkout	6.45	6.62	0.473
Amount Paid ($)	**38.83**	**39.69**	**0.663**
Full-Service	54.60	56.38	0.662
Express	23.35	25.89	0.093*
Self-Checkout	19.64	19.87	0.750
N Stores	20	33	–
N Stores w/Self-checkout	9	16	–

Note: 2011 is in the pre-policy period for all stores in the sample. Asterisks indicate the following: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Author's calculations from the scanner data.

2.3 Empirical Model: Event Study Design

I estimate the causal effect of DCB policies on checkout duration using an event study design. This approach can be thought of as unpacking a difference-in-differences design. Since each treated store can have a unique pre-/post-period, the event study model reorders the panel data to align the treatment events so that the differences in outcomes between treated and control stores can be plotted over event-time.

I average the transaction-level scanner data to the store-week level and employ the following event study regression model:

$$Y_{sjw} = \sum_{l=-24}^{24} \beta_l D_{l,jw} + \beta_x X_{sjw} + \theta_{sj} + \delta_w + \epsilon_{sjw} \tag{2.1}$$

where Y_{sjw} is the outcome variable for store s in jurisdiction j and week-of-sample w, X_{sjw} is a set of control variables, θ_{sj} is a vector of store fixed effects, and δ_w is a vector of week-of-sample fixed effects. $D_{l,jw}$ is a dummy variable equaling one if jurisdiction j in week w implemented a DCB policy l weeks ago, with $l = 0$ denoting the week of implementation. The endpoints are binned, with $D_{24,jw} = 1$ for all weeks in which it is 24 weeks or more since DCB policy implementation and, similarly, $D_{-24,jw} = 1$ for all weeks in which it is 24 weeks

Table 2.3: Average Store-Shift Characteristics in 2011

	Control	Treat	P-value on Diff.
Transactions per shift (#)	**542.98**	**566.78**	**0.620**
Full-Service (share)	0.56	0.52	0.261
Express (share)	0.56	0.52	0.261
Self-Checkout (share)	0.15	0.15	0.991
Amount spent per shift ($)	**$20,601.71**	**$21,811.17**	**0.641**
Full-Service (share)	0.76	0.72	0.081*
Express (share)	0.17	0.22	0.201
Self-Checkout (share)	0.07	0.07	0.970
Items bought per shift (#)	**6,952.38**	**7,380.48**	**0.614**
Full-Service (share)	0.77	0.72	0.071*
Express (share)	0.17	0.21	0.187
Self-Checkout (share)	0.07	0.07	0.954
Registers open per shift (#)	**7.89**	**8.54**	**0.515**
Register Capacity (#)	**10.25**	**10.64**	**0.700**
N Stores	20	33	–
N Stores w/Self-checkout	9	16	–

Note: 2011 is in the pre-policy period for all stores in the sample. Asterisks indicate the following: * p < 0.10, ** p <0.05, *** p <0.01. *Source*: Author's calculations from the scanner data.

or more until implementation.[33] The week prior to implementation ($l = -1$) is the omitted category. Store fixed effects control for time-invariant store level characteristics (i.e., store size, number of registers, types of departments offered). Week-of-sample fixed effects control for variation over time that effect all stores (i.e., holidays and seasons).

The β_l vector is the parameter of interest, as it traces out the adjustment path from before the DCB policies to after. I hypothesize that customer, cashier, and store learning will result in more complex dynamics than a simple discrete shift in the outcome variable (as would be implied by a model that replaced the $D_{l,jw}$ variables with a single indicator variable for the post-policy period). Customers must learn how to respond to the policy and change their habits (i.e., bring more bags from home, buy paper or reusable bags at checkout). Cashiers must alter their checkout procedures. Store managers may reoptimize

[33] I choose ±24 weeks as endpoints because I hypothesize that 24 weeks (or roughly half a year) is enough time to witness learning. I also bin at +24 weeks because stores that implement policies later in the sample period mechanically have fewer post-policy weeks than stores with early implementation dates. While all textcolorblack33 treated stores have at least fifteen weeks in the post-policy period, only 28 stores have thirty weeks, only 23 stores have sixty weeks, only 13 stores have eighty weeks, and so on. Thus, binning the endpoints at 24 weeks provides ample time for measuring learning without losing too many of the treated stores. I will also examine whether the results are robust to binning at -48 and +96 weeks.

the number of lanes open and the placement of cashiers as to keep lines to a minimum. All of these behaviors may change over time as customers, cashiers, and stores learn, adapt, and circumvent the new policies.

Therefore, I expect that the effects of the policy will be greater in the initial weeks, and will diminish over time (i.e., β_0 will be greater in magnitude than β_{24}). To test this formally, I will use two Wald tests. In the first test, the null hypothesis is that all coefficients in the post-policy are equal (i.e., $\beta_0 = \beta_1 = \beta_2 = ... = \beta_{24}$) and in the second test, the null hypothesis is that the coefficient for the first week of the policy is equal to the coefficient for all weeks 24 or more after the policy (i.e., $\beta_0 = \beta_{24}$). Rejecting these hypotheses would provide evidence of learning.

The identifying assumption of the model is that, absent the DCB policies, outcomes at the treated stores would have remained similar to the control stores. Underlying trends in the outcome variable correlated with DCB policy enactment are the most likely violation of this assumption. Part of the appeal of event study designs is that the pre-policy portion of the β_l vector provides a check against this possible violation. If DCB policies are unassociated with underlying trends, there should be no trend in the β_l vector in the pre-policy period.

The primary outcome variables I use for Y_{sjw} will be (1) logged average transaction duration, measured in minutes, and (2) average number of transactions completed per 1:00-4:00pm shift. I examine additional outcome variables as well, such as average share of transactions purchasing paper and reusable bags, and the number of registers open.

2.4 Results

Average Effects of DCB Policies on Transaction Duration

The figures in this section present the results from the estimation of event study Equation 2.1, where the $\hat{\beta}_l$ point estimates and 95% confidence intervals are displayed graphically.[34] Unless specified otherwise, I cluster the standard errors two ways—by jurisdiction (45) and by week-of-sample (177)—to allow for spatial and temporal correlation in the data.[35]

In Figure 2.3, the transaction-level scanner data are averaged to the store-week level, for a total of 9,381 observations. The outcome variable, Y_{sjw}, is logged average transaction duration, which means the $\hat{\beta}_l$ point estimates measure the percent difference in transaction duration between control and treated stores l weeks from the DCB policy implementation. Panel (a) displays the results for the simplest specification, which includes the event study indicators, store fixed effects, and week-of-sample fixed effects. Variations in grocery shopping demand by store and week-of-sample (such as from local festivals and sporting events) may influence checkout duration, and these variations are not absorbed by the store and

[34]I estimate all fixed-effect equations in STATA using the command `reghdfe` (Correia, 2014).

[35]Estimating a model that allows for spatial correlation up to 12 km and temporal correlation up to 8 weeks using spatial errors—as described by Conley (2008) and implemented using code from Hsiang (2010) and Fetzer (2014)—does not change the significance of the results.

week-of-sample fixed effects. To account for grocery shopping demand which varies by store and week, the specification in panel (b) additionally includes control variables, X_{sjw}, for the average number of items purchased per transaction, the average dollar amount spent per transaction, and the share of transactions purchasing (a) produce, (b) fresh meat and seafood, (c) dairy and refrigerated, (d) frozen, (e) bakery and deli, (f) shelf-stable food, (g) alcohol and tobacco, (h) infant/toddler, (i) floral department, and (j) pet items. In Section 2.5, I verify that these control variables are not bad controls—i.e., the average number and types of items purchased does not change with the implementation of DCB policies.

In panel (a), I find strong evidence that the DCB policies lead to increased average transaction duration. The slowdown starts in the first week of the policy with $\hat{\beta}_0 = 0.037$, which means that the average transaction duration at treated stores is 3.7% longer during the first week of a DCB policy. The slowdown fluctuates slightly over time, peaking with $\hat{\beta}_4 = 0.048$ and ending with $\hat{\beta}_{24} = 0.025$. The $\hat{\beta}_{24}$ coefficient indicates that for all weeks in which it has been 24 or more weeks since DCB policy implementation, transactions at treated stores remain 2.5% longer than at control stores.[36] The majority of the post-policy $\hat{\beta}_l$ coefficients are significantly greater than zero at the 10% significance level. Importantly, only one of the pre-policy $\hat{\beta}_l$ coefficients is significantly different from zero, which provides evidence in favor of the identifying assumption that transaction durations at treated stores were not trending differently than at control stores before the DCB policies went into effect. Panel (b) shows that the inclusion of control variables does not greatly alter the $\hat{\beta}_l$ coefficients. Unless otherwise specified, I will use the full model specification with control variables in the remainder of the paper.

Using Wald tests to compare the event study coefficients in Figure 2.3b, I can reject that all $\hat{\beta}_l$ coefficients in the post-policy period are equal,[37] however, I cannot reject that $\hat{\beta}_0 = \hat{\beta}_{24}$.[38] These results suggest that DCB policies lead to a persistent increase in transaction duration over the sample period relative to control stores. In other words, I do not find evidence of transaction durations returning to pre-policy levels over time as customers, cashiers, and stores grow accustomed to the DCB policies.

A potential concern is that 24 weeks (or roughly half a year) is not enough time to witness learning. In Figure 2.4, I explore whether the effects of DCB policies on transaction duration lessen over time if the event study model is binned at –48 and +96 weeks (i.e., roughly 1 year before and 2 years after) instead of ±24 weeks. I find the 3% slowdown in transactions duration persists even when the event study is binned at –48 and +96 weeks. However, the $\hat{\beta}_l$ estimates grow noisier after D_{73}, when the number of treated stores in the sample drops to 13.

In addition to the event study model in Equation 2.1, I estimate the following difference-in-differences (DID) model:

$$Y_{sjw} = \beta_D D_{jw} + \beta_x X_{sjw} + \theta_{sj} + \delta_w + \epsilon_{sjw} \tag{2.2}$$

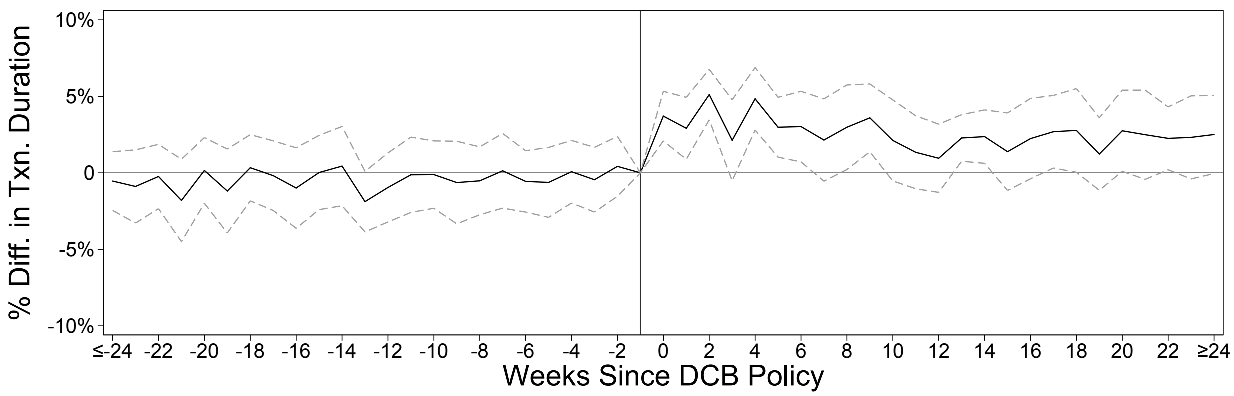
[36] The numerical regression output for Figure 2.3a can be found in Appendix Table A.2.

[37] F(24, 44) = 7.02, p-value = 0.000.

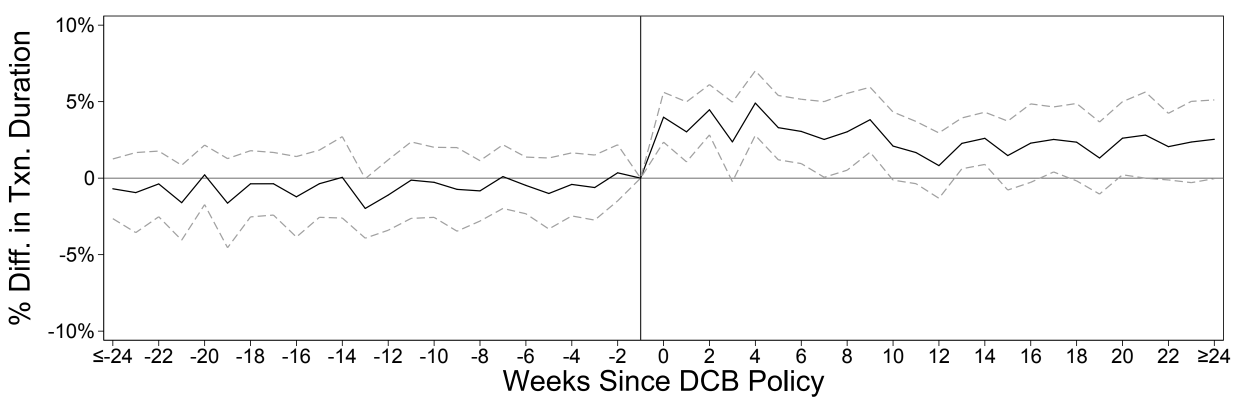
[38] F(1, 41) = 1.63, p-value = 0.208.

Figure 2.3: Effect of DCB Policies on Transaction Duration (*Store-Week Averages*)

(a) Logged Transaction Duration—Without Control Variables

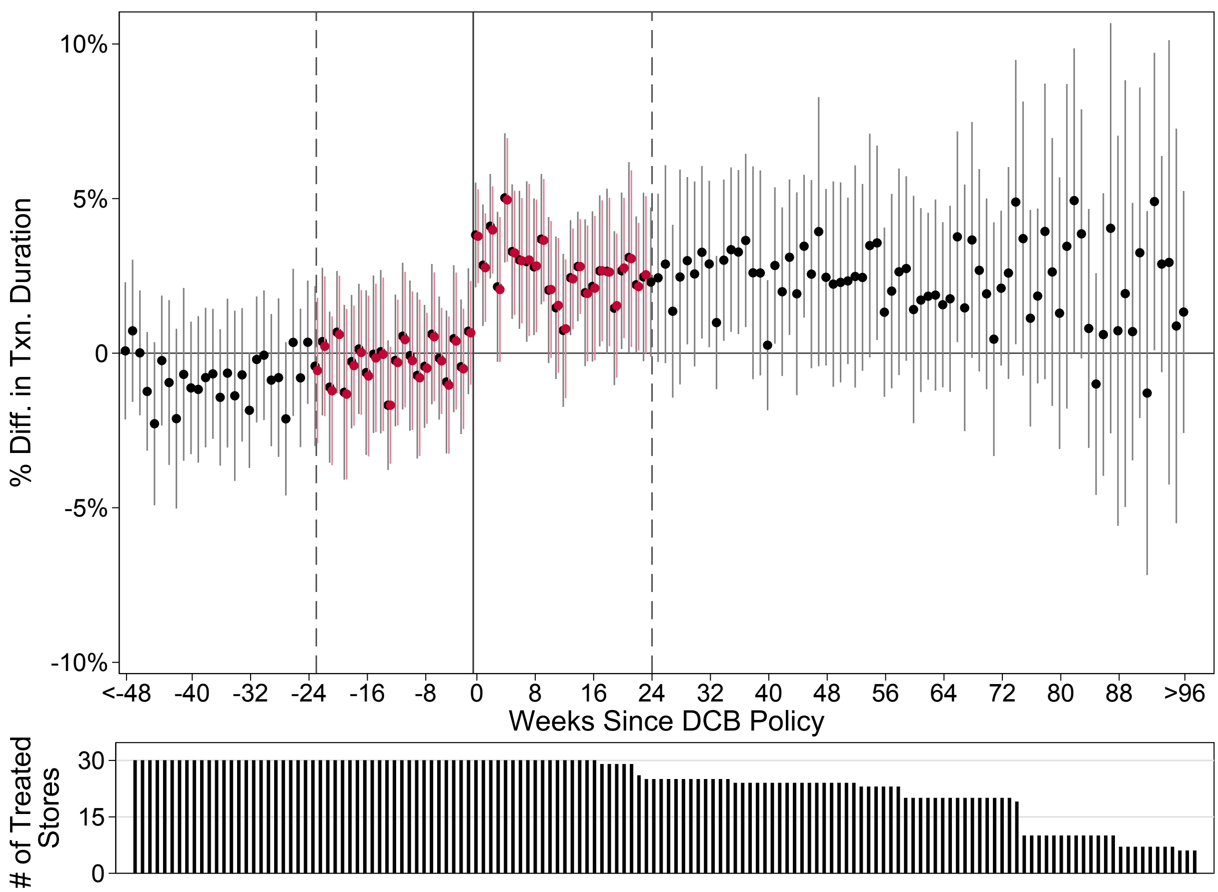


(b) Logged Transaction Duration—With Control Variables



Note: The figure panels display the $\hat{\beta}_l$ coefficient estimates from event study Equation 2.1. The dependent variable is logged average transaction duration, measured in minutes, in store s, jurisdiction j, and week-of-sample w. Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample. Panel (a) presents the specification of Equation 2.1 with event study indicators, store fixed effects, and week-of-sample fixed effects. The specification in panel (b) additionally includes control variables, X_{sjw}, for average transaction size, average transaction expenditures, and the share of transactions purchasing each of the following items—produce, meat/seafood, dairy/refrigerated, frozen, bakery/deli, shelf-stable food, alcohol/tobacco, infant/toddler, floral department, and pet items.

Figure 2.4: Effect of DCB Policies on Transaction Duration, with Extended Endpoints (*Store-Week Averages*)



Note: The top panel of the figure displays the $\hat{\beta}_l$ event study estimates, in black, from the extended event study equation: $Y_{sjw} = \sum_{l=-49}^{97} \beta_l D_{l,jw} + \beta_x X_{sjw} + \theta_{sj} + \delta_w + \epsilon_{sjw}$. In red, between the dashed lines, are the β_l estimates from Equation 2.1—same as in Figure 2.3a—where the event study endpoints are instead binned at ± 24 weeks. The dependent variable is logged average transaction duration, measured in minutes, in store s, jurisdiction j, and week-of-sample w. Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample. The bottom panel presents a bar plot for the number of treated stores with $D_{l,sw} = 1$. Stores that implement policies later in the sample period mechanically have fewer post-policy weeks than stores with early implementation dates. This plot shows that while all 33 treated stores have $D_{15,sw} = 1$, only 7 treated stores have $D_{96,sw} = 1$.

where D_{jw} is now a single dummy variable equal to 1 when a DCB policy is in effect in jurisdiction j and week-of-sample w, instead of the set of event study dummy variables. The results are presented in column (1) of Table 2.4. I estimate $\hat{\beta}_D = 0.033$ (p-value = 0.000), which is consistent with the event study results in Figure 2.3.

What are the implications of a 3% slowdown? A 3% increase in transaction duration means that the median 2 minute transaction is approximately 3.6 seconds slower. While 3.6 seconds might seem like a trivial amount of time, when aggregated across all shopping trips made per year, this time cost becomes substantial. In the United States, an estimated 11.9 billion grocery shopping trips are made annually,[39] meaning 3.6 seconds per grocery shopping trip would equal 11.9 million additional grocery shopping hours per year. That is equivalent to 1,358 years.

Heterogeneity by Transaction Size

Given that I find a statistically significant and persistent 3% slowdown in transaction duration due to DCB policies on average, I next investigate mechanisms behind the slowdown, and in particular, whether the effects of the policies are heterogeneous by characteristics of the transactions. First, I examine whether the effects of DCB policies on transaction duration are heterogeneous by transaction size—i.e., the number of items scanned in a transaction. To do this, I split the roughly 9.8 million transactions in my sample into four equal size quartiles: Q1=transactions with 3 or fewer scans, Q2=transactions with 4-8 scans, Q3=transactions with 9-18 scans, and Q4=transactions with 19 or more scans.

To understand how the size of transactions interacts with the effect of DCB policies, I estimate Equation 2.1 by size quartile (i.e., I average the transaction-level scanner data to the store-by-week-by-size-quartile level). Figure 2.5 presents the results of the full model specification estimated separately for each size quartile in ascending order. In this figure, the outcome variable, Y_{sjw}, is again logged average transaction duration, which means the $\hat{\beta}_l$ point estimates measure the percent difference in transaction duration between treated and control stores l weeks from the DCB policy implementation. In panels (a) and (b), I find no statistically significant slowdown in transaction duration due to the DCB policies for the smallest two quartiles of transactions. Conversely, in panels (c) and (d), I find strong evidence that the policies cause slowdowns for transactions in the largest two quartiles. During the first week of the policies, Q3 transactions are 4.5% longer ($\hat{\beta}_0 = 0.045$) than prior to the policy. The $\hat{\beta}_l$ coefficients decline over time with $\hat{\beta}_{24} = 0.027$. For Q4 transactions, the slowdown starts with $\hat{\beta}_0 = 0.055$ and continues through the end of the sample with $\hat{\beta}_{24} = 0.037$. Using Wald tests for Q3 and Q4, I find mixed evidence that the slowdown for larger transactions lessens over time. While I cannot reject that $\hat{\beta}_0 = \hat{\beta}_{24}$ at the 10%

[39] Hamrick et al. (2011) estimate how much time Americans spend on food and find that the average adult in the U.S. grocery shops once every 7.2 days. Given there are roughly 235 million adults in the U.S. (2010 U.S. Census), this equates to 11.9 billion grocery shopping trips per year.

Table 2.4: Effect of DCB Policies on Transactions per Shift and Line Length (*Store-Week Averages*)

	(1) Txn. Duration (mins)	(2) Txns. per Shift (#)
Levels (Y_{sjw})		
Ban Effective Dummy	0.064***	-18.226***
	(0.017)	(6.047)
Percent (lnY_{sjw})		
Ban Effective Dummy	0.033***	-0.030***
	(0.008)	(0.010)
Num of Obs.	9381	9381
Standard Errors	Cluster	Cluster
Covariates X_{sjw}	Yes	Yes
Store FE	Yes	Yes
Week-of-sample FE	Yes	Yes
Mean Y_{sw} (2011)	2.045	556.829
Changes in Line Length		
Ave. $\uparrow$ customers in line ($\frac{\lvert\hat{\beta}_D\rvert}{2}$)		9.113
Registers open, post-policy		8.931
Ave. $\uparrow$ customers in line, per open register		1.020
Registers available		10.491
Ave. $\uparrow$ customers in line, per available register		0.869

Note: The top half of the table presents the results from Equation 2.2, with the outcome variable estimate in both levels and logs. In column (1), the outcome variable is the average number of transactions completed per 3 hour shift in store s, jurisdiction j, and week w. In column (2), the outcome variable is the average transaction duration, measured in minutes, in store s, jurisdiction j, and week w. Standard errors are in parentheses. Standard errors are estimated using two-way error clustering at the policy jurisdiction and week-of-sample level. Asterisks indicate the following: $*p < 0.10$, $**\,p < 0.05$, $***p < 0.01$.

The bottom half of the table reports the $\frac{|\hat{\beta}_D|}{2}$ estimate from the specification in levels. This estimate is the average increase in customers in line due to the policy change and it is used to calculate the additional number of customers in line *per register* either (i) given the average number of registers open in the post-policy period, or (ii) if all existing registers were open.

significance level, I can reject that all coefficients in the post-policy period are the same as one another.[40]

Together these results suggest that the impact of DCB policies increases with the transaction size.[41] In other words, the policies do not impose a fixed time cost but instead the time cost depends on the number of items purchased. Why do DCB policies affect transactions of various sizes heterogeneously? One hypothesis is that transactions of different sizes choose different types and quantities of carryout bags. To understand how bag choice varies by transaction size, I estimate Equation 2.1 separately for each transaction size quartile, with Y_{sjw} being (i) the share of transactions paying for at least one paper bag in the post-policy period and (ii) the share of transactions purchasing at least one reusable bag.[42] Figures 2.6 and 2.7 present the results.

As one would expect, in all panels of Figure 2.6 I find a sharp and permanent increase in the share of customers purchasing paper bags which is contemporaneous with DCB policy implementation. Since paper bags were available but not sold before the DCB policies, this figure reassures me that I have the correct timing of the DCB policy implementation. Additionally, I find that the share of transactions where consumers purchase paper bags increases with transaction size, with approximately 6% of Q1 transactions, 23% of Q2 transactions, and 32% of both Q3 and Q4 transactions choosing to purchase paper bags in the first week of the policy. In each panel, these shares decrease by roughly 5 percentage points over time.[43]

[40] If I estimate the DID model in Equation 2.2 instead of the event study model in Equation 2.1, I find slowdowns due to the DCB policies for the largest three quartiles ($\hat{\beta}_D^{Q2}$=0.021, $\hat{\beta}_D^{Q3}$=0.034, and $\hat{\beta}_D^{Q4}$=0.051), all statistically different from zero at the 10% level. Since larger transactions have longer checkout durations to begin with, this translates to Q2 transactions being 2 seconds slower, Q3 transactions being 4 seconds slower and Q4 transactions being 8 seconds slower on average.

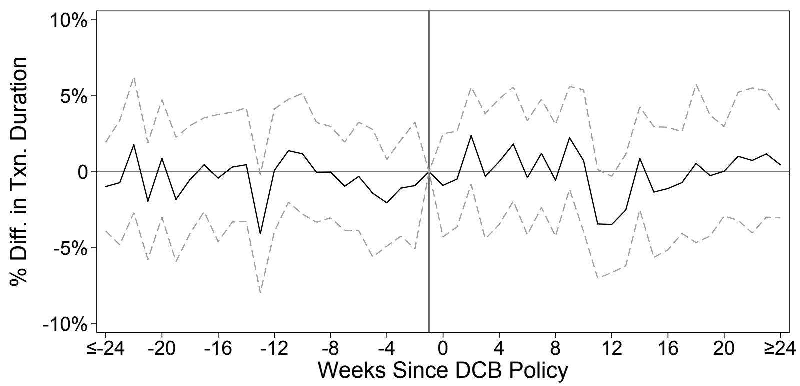
[41] To formally test whether the event study results differ by transaction size, I perform a Chow Test (Chow, 1960)—comparing the residual sum of squares from the separate transaction quartile regressions to the residual sum of square of the whole sample. I calculate an F-statistic of 383.107 and can thus reject, at the 1% significance level, the null hypothesis that transaction size has no impact on the effects of DCB policies.

[42] It is important to note that in the scanner data I see whether or not a transaction pays the paper bag fee, but not how many paper bags are purchased.

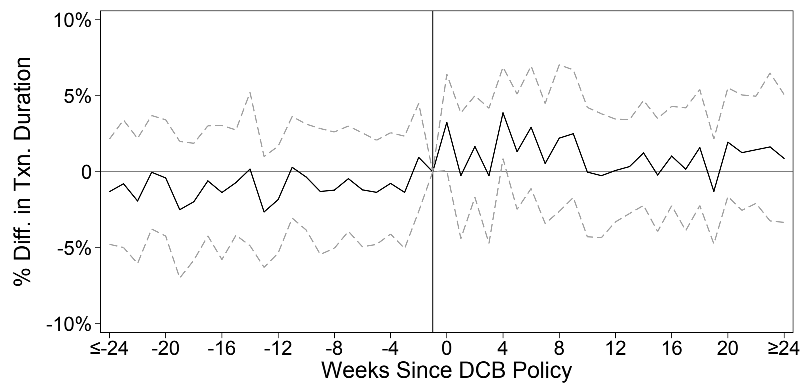
[43] Since DCB policies stipulate that customers using food assistance program benefits—such as Supplemental Nutrition Assistance Program (SNAP) and Women, Infants, and Children (WIC) benefits—may obtain paper bags without paying a bag fee, the shares reported in Figure 2.6 are a lower bound for the share of customers obtaining paper bags at checkout. Out of 38.8 million Californians, 4.4 million received SNAP benefits in 2015—roughly 11 percent of Californians ("Just the Facts: The CalFresh Food Assistance Program." *Public Policy Institute of California*. Online, *accessed May 17, 2016*). Thus the scanner data may miss a sizable chunk of paper bag use. Taylor and Villas-Boas (2016b) use observational data collected in-store and find a higher share of transactions obtaining paper bags, between 30 and 40 percent, when a California DCB policy is in effect. This discrepancy may also be due to cashiers occasionally forgetting to charge the fee.

Figure 2.5: Heterogeneity in Effect of DCB Policies on Transaction Duration, by Transaction Size Quartile (*Store-Week Averages*)

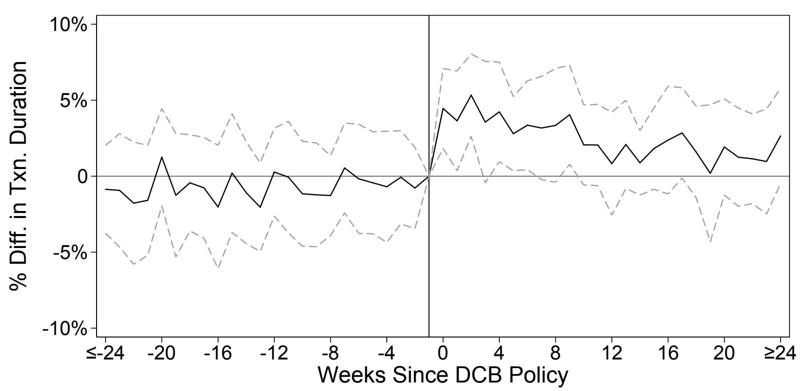
(a) Logged Transaction Duration—Q1 (Scans < 4)



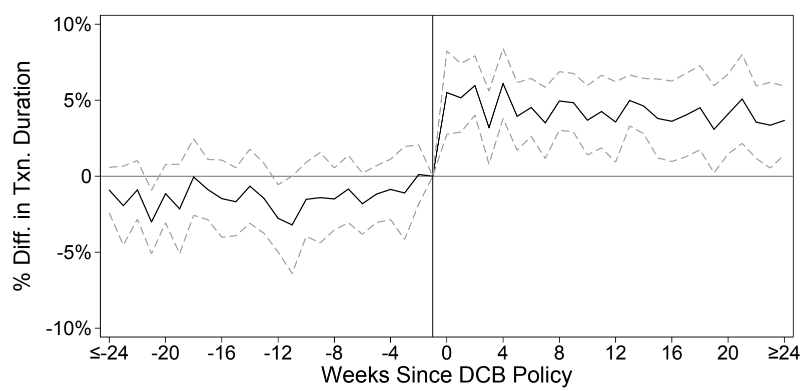
(b) Logged Transaction Duration—Q2 (Scans = 4-8)



(c) Logged Transaction Duration—Q3 (Scans = 9-18)

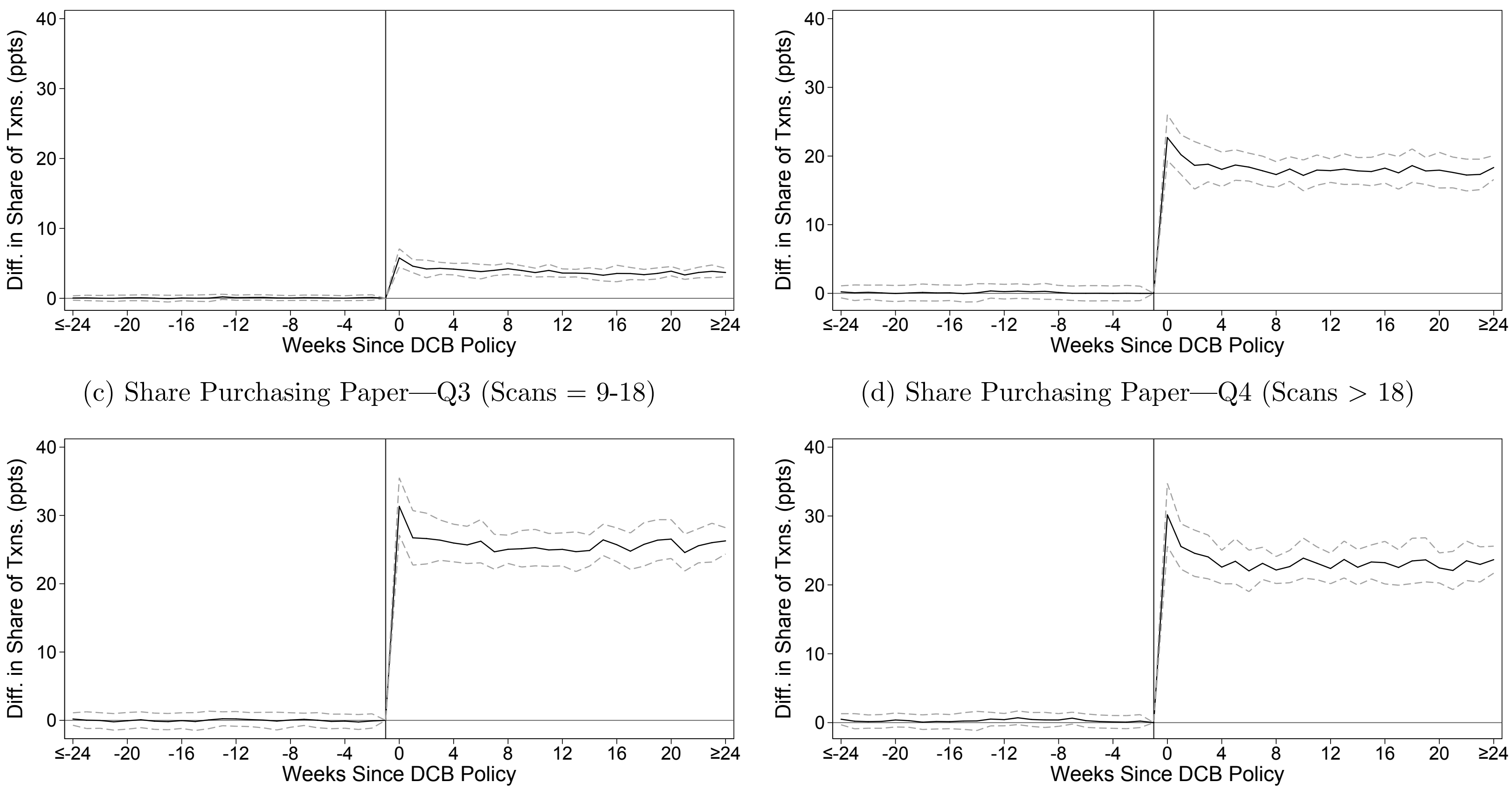


(d) Logged Transaction Duration—Q4 (Scans > 18)



Note: The figure panels display the $\hat{\beta}_1$ coefficient estimates from full specification of event study Equation 2.1. The dependent variable is logged average transaction duration, measured in minutes, in store s, jurisdiction j, and week-of-sample w, by transaction size quartile with the smallest transaction quartile in panel (a) and the largest transaction quartile in panel (d). Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample.

Figure 2.6: Effect of DCB Policies on Share of Transactions Purchasing Paper Bags, by Transaction Size Quartile (*Store-Week Averages*)



(a) Share Purchasing Paper—Q1 (Scans < 4)

(b) Share Purchasing Paper—Q2 (Scans = 4-8)

(c) Share Purchasing Paper—Q3 (Scans = 9-18)

(d) Share Purchasing Paper—Q4 (Scans > 18)

Note: The figure panels display the $\hat{\beta}_1$ coefficient estimates from the full specification of event study Equation 2.1. The dependent variable is share of transactions in store s, jurisdiction j, and week-of-sample w purchasing paper bags by transaction size quartile, with the smallest transaction quartile in panel (a) and the largest transaction quartile in panel (d). Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample.

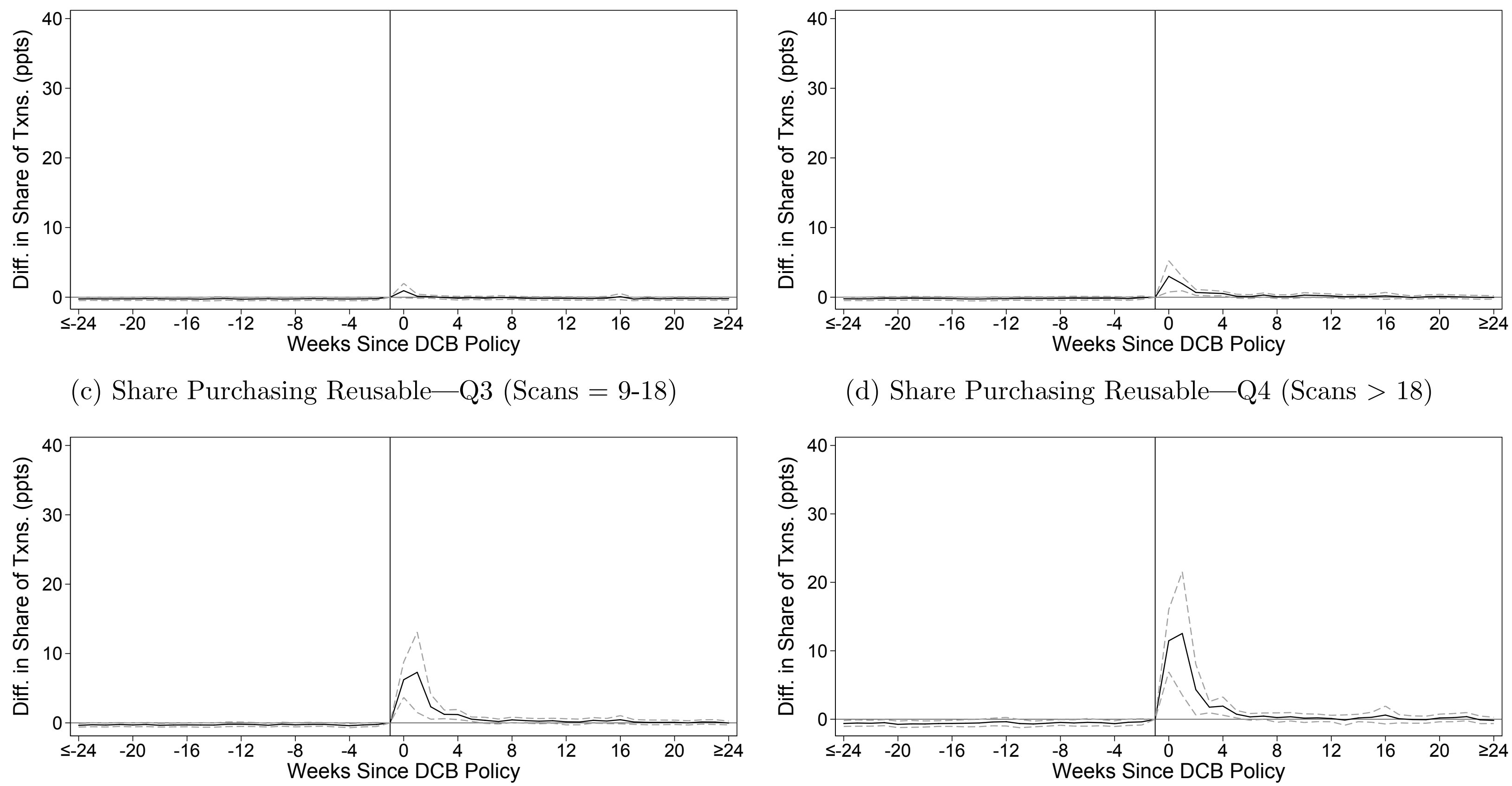
Figure 2.7: Effect of DCB Policies on Share of Transactions Purchasing Reusable Bags, by Transaction Size Quartile (*Store-Week Averages*)

(a) Share Purchasing Reusable—Q1 (Scans <4)

(b) Share Purchasing Reusable—Q2 (Scans $=$ 4-8)

(c) Share Purchasing Reusable—Q3 (Scans $=$ 9-18)

(d) Share Purchasing Reusable—Q4 (Scans >18)



Note: The figure panels display the $\hat{\beta}_1$ coefficient estimates from the full specification of event study Equation 2.1. The dependent variable is the share of transactions in store s, jurisdiction j, and week-of-sample w purchasing reusable bags, by transaction size quartile with the smallest transaction quartile in panel (a) and the largest transaction quartile in panel (d). Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample.

For reusable bags in Figure 2.7, I find a temporary increase in purchases of reusable bags when the DCB policies are implemented. Reusable bags are sold at the supermarket chain both before and after the DCB policies.[44] As with paper bags, the share of transactions choosing to buy reusable bags increases with transaction size, with less than 1% of Q1 transactions, 3% of Q2 transactions, 6% of Q3 transactions, and 11% of Q4 transactions choosing to purchase reusable bags in the first week of the policy. However, these increases quickly retreat, and by eight weeks after the DCB policy implementation the share of transactions purchasing reusable bags is indistinguishable from zero. This pattern is consistent with customers reusing the reusable bags they purchase in the first couple weeks of the policy.

Heterogeneity by Both Transaction Size and Paper Bag Choice

To explore how transaction size and customer bag choice interact to influence the effects of DCB policies on transaction duration, I estimate Equation 2.1 by size quartile and by whether a transaction purchased a paper bag (i.e. the transaction-level scanner data are averaged to the store-by-week-by-size-quartile level for those that purchase paper at treated stores in the post-policy period and for those that do not). Figure 2.8 presents the results, with transactions not purchasing paper bags on the left and transactions purchasing paper bags on the right.[45]

I find a stark difference in the effect of the policies between transactions with and without paper bag purchases, especially in the larger quartiles. For Q2 transactions that do not pay the paper bag fee, I find no effect of the policies. For Q2 transactions that do pay the paper bag fee, the post-policy $\hat{\beta}_l$ coefficients are positive on average, but the majority are not statistically different from zero. The same is true for Q3 transactions that do not purchase paper bags. However, for Q3 transactions that do purchase paper bags, the $\hat{\beta}_l$ coefficients in the post-policy period are greater in magnitude (e.g., $\hat{\beta}_0 = 0.078$ and $\hat{\beta}_{24} = 0.052$) and statistically different from zero. For the largest transactions in Q4, the effects of DCB policies on transaction duration are even stronger. DCB policies lead to a 2-4% slowdown in transaction duration for Q4 transactions not purchasing paper bags, and to a 7-12% slowdown for transactions that do purchase paper bags.

Overall, these results suggest that customers purchasing paper bags experience larger slowdowns than those not purchasing paper bags of the same transaction size. It should be noted that these results are not identifying a causal effect of choosing paper bags on transaction duration, as I do not randomly assign who gets paper and who does not. Customers who choose to pay for paper bags could be inherently slower than those that do not.[46] Yet since I run the regressions within transaction size quartile and control for the average amount spent and types of items purchased, conditional on observables these estimates are

[44] The prices of reusable bags do not differ between treated and control stores and they also do not change when the DCB policies go into effect. I find this both in the scanner data and during in-store visits.

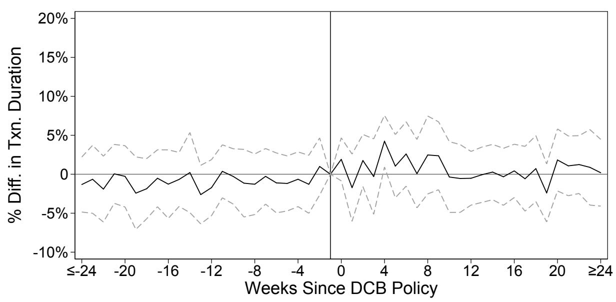
[45] I do not present the results for Q1 because so few of these transactions purchase paper bags.

[46] In Appendix A.2, I show that paper bag use is positively correlated with income, transaction size, and purchasing more expensive items.

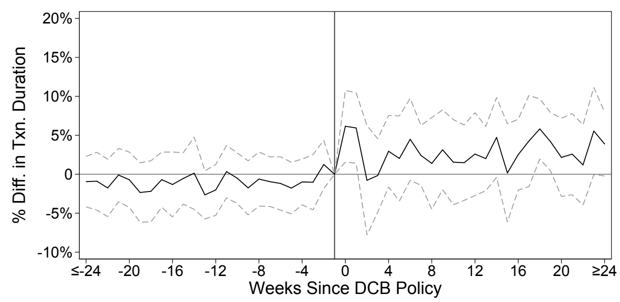
Figure 2.8: Heterogeneity in Effect of DCB Policies on Transaction Duration, by Transaction Size Quartile and Paper Bag Purchase (*Store-Week Averages*)

Without paper bags | **Paid paper bag fee**

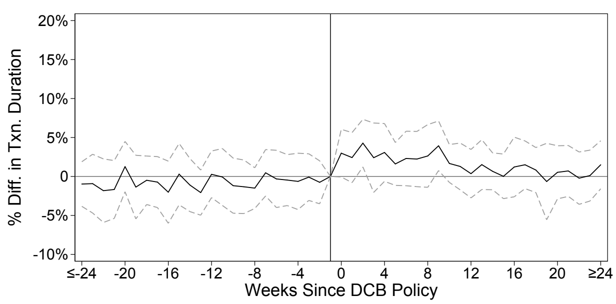
(a) Log Txn Duration—Q2



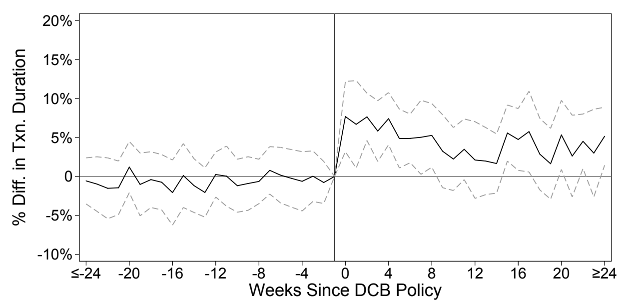
(b) Log Txn Duration—Q2



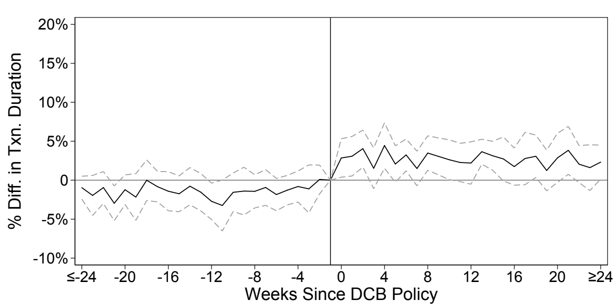
(c) Log Txn Duration—Q3



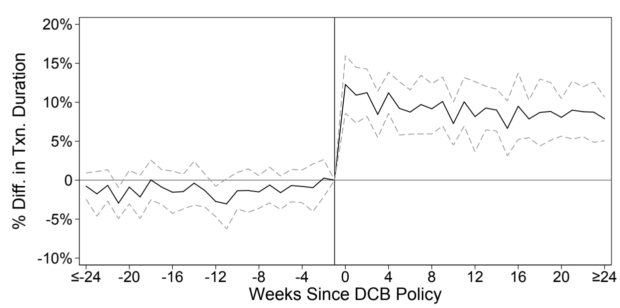
(d) Log Txn Duration—Q3



(e) Log Txn Duration—Q4



(f) Log Txn Duration—Q4



Note: The figure panels display the $\hat{\beta}_1$ coefficient estimates from the full specification of event study Equation 2.1. The dependent variable is logged average transaction duration, measured in minutes, in store s, jurisdiction j, and week-of-sample w, by transaction size quartile and paper bag use. Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample.

quite suggestive that paper bag choice is a mechanism behind the slowdown. Which raises the following question: Is the slowdown caused by the additional action of paying for paper bags, or are paper bags simply slower to pack than other types of bags? Given that cashiers enter in the bag fee code once per transaction, no matter the transaction size, one might expect the percent change in transaction duration from entering the fee to be larger for smaller transactions than for larger transactions. This is not what I find here, where the larger transactions experience the larger percent changes. These results provide evidence that bag type is an important mechanism behind the persistent slowdown, with paying for paper bags additively slower than getting "free" plastic bags.

2.5 Alternative Mechanisms Behind Slowdown

In the event study results presented above, I find that DCB policies lead to statistically significant and persistent increases in transaction duration. Additionally, I find that the effects of DCB policies are greater for larger transactions and for transactions paying the paper bag fee. In this section I rule out three alternative mechanisms for why DCB policies lead to checkout slowdowns.

Mechanism 1: Do DCB policies alter what customers purchase?

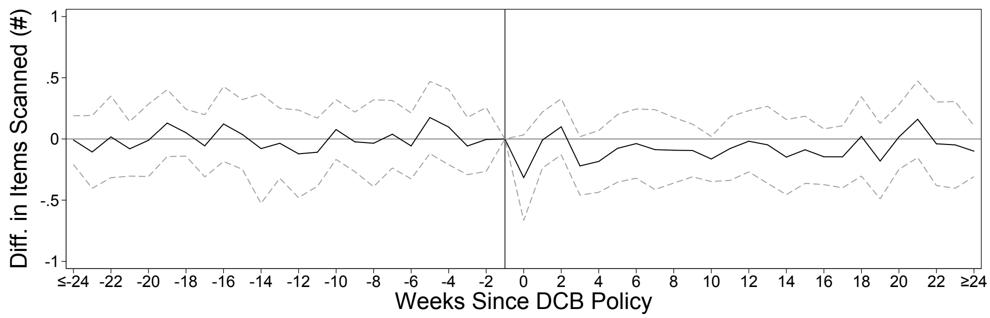
Are the slowdowns in transaction duration driven by changes in what customers buy when the DCB policies go into effect? In Figure 2.9, I examine whether the number of items purchased and the amount spent per transaction changes when DCB policies are implemented. I estimate the simplest specification of Equation 2.1, with only store and week-of-sample fixed effects. Each panel of Figure 2.9 has a different outcome variables, Y_{sjw}, at the store-week level: (a) the average number of items scanned per transaction, not including checkout bags, (b) the average dollars spent per transaction, and (c) the average dollars spent per item.[47]

In panels (a) and (b), I do not find evidence that DCB policies lead to changes in the average number of items purchased or in the average amount spent per transaction, with the majority of $\hat{\beta}_l$ coefficients statistically indistinguishable from zero. In panel (c), I find some evidence of a temporary dip in the average amount spent per item. Specifically, in the second week of the policy, the average amount spent per item is 2.4 cents lower than at control stores ($\hat{\beta}_1 = -0.024$). While I do not observe the size or volume of items purchased, this is consistent with the hypothesis that DCB policies alter the size of the items purchased, with customers preferring smaller (and less expensive) items when they need to pay for, or remember, checkout bags. However, since this change is temporary and quite small in

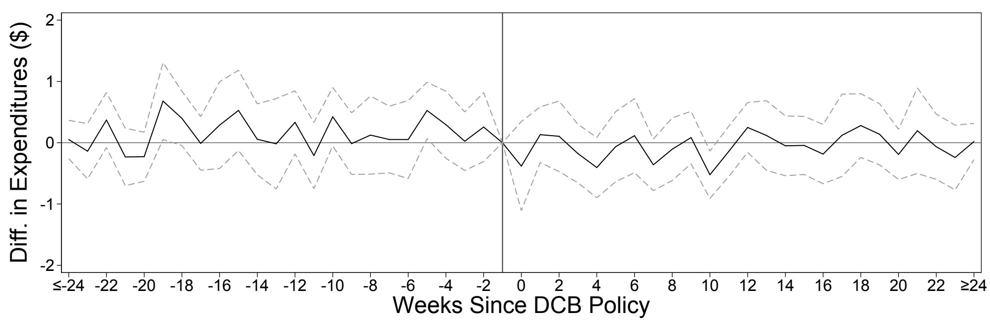
[47] The dollars spent variable is created by summing up the individual amounts spent per item in a transaction, and therefore, it does not include sales tax. Several point of sale line items, including the line item for purchasing a paper bag and for making a donation to charity, do not include an amount spent. Since the amount spent variable does not include paper bags purchased, I measure the average amount paid per item as the amount paid per transaction divided by the number of items scanned not including checkout bags.

Figure 2.9: Effect of DCB Policies on Number of Items Scanned and Amount Spent (*Store-Week Averages*)

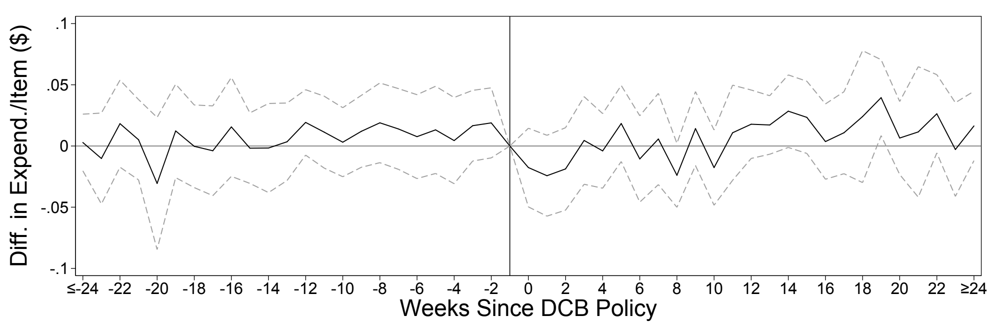
(a) Number of Items Scanned per Transaction, without Bags



(b) Amount Spent per Transaction ($)



(c) Amount Spent per Item ($)



Note: The figures display the $\hat{\beta}_l$ coefficient estimates from the simple specification of event study Equation 2.1, controlling for only store and week-of-sample fixed effects. The dependent variables are (a) the average number of items scanned per transaction in store s, jurisdiction j, and week w, not including checkout bags, (b) the average amount spent per transaction in store s, jurisdiction j, and week w, and (c) the average amount spent per item in store s, jurisdiction j, and week w. Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample.

magnitude,[48] it is unlikely to be the mechanism behind the persistent slowdown in checkout transactions.

I also estimate equations 2.1 and 2.2 with the outcome variable being the share of transactions in store s, jurisdiction j, and week-of-sample w purchasing items in the following categories: 1) produce, 2) meat and seafood, 3) dairy and refrigerated, 4) frozen, 5) bakery and deli, 6) shelf-stable food, 7) alcohol and tobacco, 8) baby, 9) floral, and 10) pet. Except for produce, I find no significant changes due to the DCB policies in the share of transactions purchasing these items. While I find a 0.4 percentage point decrease in the share of transactions purchasing produce (statistically significant at the 10% level), given that 52 percent of transactions in the pre-policy period purchase produce, this is a relatively small change in the share.[49]

Mechanism 2: Do DCB policies alter where customers checkout?

Along with choosing how many and what type of groceries to buy, customers also choose at which register to queue. In Figure 2.10, I first estimate Equation 2.1 with the outcome variable being either the share of transactions in cashier-operated registers or in self-checkout registers. Second, to see whether stores open more registers in response to the DCB policies, I also estimate Equation 2.1 with the outcome variable being the average number of registers open for each of the register types.

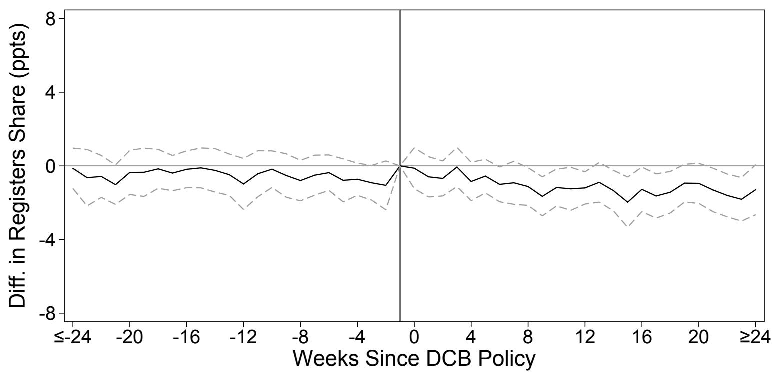
The results in Figure 2.10 are organized with register share as the outcome variable in the left-hand side panels and the number of registers open as the outcome variable in the right-hand side panels. Overall, I do not find large changes in either the share of transactions by registers type, or in the number of registers open, that are contemporaneous with policy implementation. In panels (a) and (c), I find that after the DCB policies are implemented, the share of transactions completed at cashier-operated registers declines slightly over time, and similarly, the share of transactions completed at self-checkout registers increases slightly. In particular, $\hat{\beta}_{24} = 0.013$ in panel (c), indicating a roughly 1 percentage point increase in the share of transactions at self-checkout lanes 24 weeks after policy implementation.

[48] The average item in 2011 costs \$3.01, so a 2.4 cent drop in price is less than a 1% change.

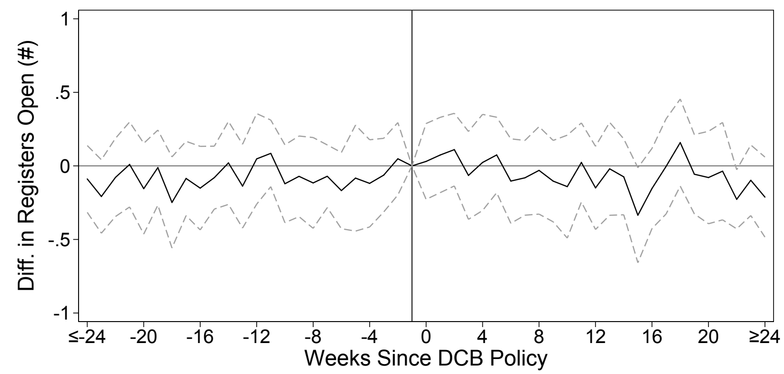
[49] The difference-in-differences estimates can be found in Appendix Table A.4. While I do not find changes in purchasing behavior when looking at these broad categories, I do find statistically significantly increases in one subcategory—garbage bags. This "plastic bag leakage" is yet another unintended consequence of DCB policies. In Chapter 3, I compare the environmental consequences of the decline of thin plastic checkout bags with the increase in purchases of other types of bags.

Figure 2.10: Effect of DCB Policies on Register Choice and Registers Open, by Register Type (*Store-Week Averages*)

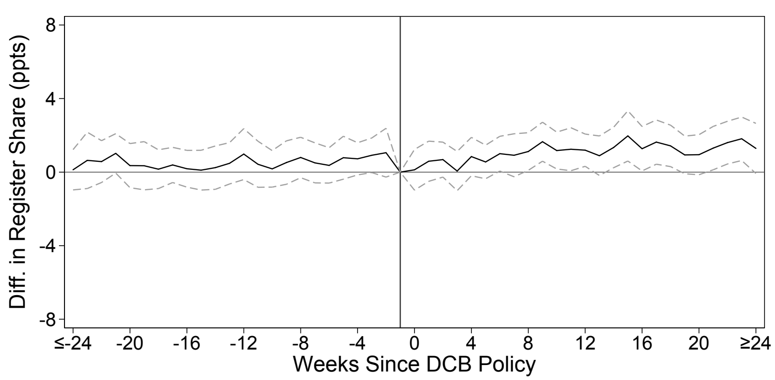
(a) Cashier-operated Register Share (full-service + express)



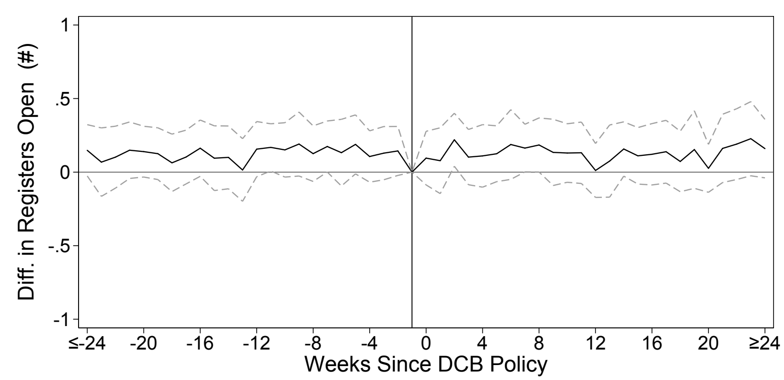
(b) Cashier-operated Registers Open (full-service + express)



(c) Self-checkout Register Share



(d) Self-checkout Registers Open



Note: The figures display the $\hat{\beta}_l$ coefficient estimates from the simplest specification of Equation 2.1. The dependent variables in panels (a) and (c) are the share of transactions by register type in store s, jurisdiction j, and week-of-sample w. The dependent variables in panels (b) and (d) are the number of registers open by register type in store s, jurisdiction j, and week-of-sample w. Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample.

This result suggests that some customers adapt to the policy by switching from full-service to self-checkout lanes. Adopting a new technology, such as self-checkout, is often spurred by dramatic events that change the effort and time of the alternatives. While transaction duration at self-checkout registers are on average 2 minutes longer than full-service transactions (as seen in Table 2.2), the self-checkout queues may be relatively shorter after the DCB policies, inducing customers to switch. Learning-by-doing might also be at play. The DCB policies may lead customers to try self-checkout registers for the first time, and having used the self-checkout once, they are more likely to do so in the future. Finally, bringing ones own bags to the store may change consumers' preferences over having other people bag their groceries. Yet, while the increased use of the slower self-checkout technology may explain some of the persistent effects of DCB policies on transaction duration, it cannot explain the initial slowdown.

In panels (b) and (d), I analyze whether stores alter the number of registers open when the DCB policies go into effect. In neither panel do I find statistically significant changes in the number of cashier-operated and self-checkout registers open. Therefore it does not appear that stores are opening more lanes to mitigate the effects of DCB policies on checkout duration. However, since my sample of transactions come from peak grocery shopping hours, where stores are operating near full register capacity, stores may be unable to open more lanes due to capital and labor constraints. In Section 2.6, I examine whether stores alter other operation behaviors—such as the number of baggers present—using data collected in-store.

Mechanism 3: Do changes in the composition of cashiers and customers drive the results?

In the store-week events studies presented in Section 2.4, I use data averaged to the store-week level in order to eliminate concerns over correlation between transactions within a store and week leading to inconsistent standard errors.[50] However, given the high turnover of cashiers and the heterogeneity of customers, using store-week data may hide changes in the composition of cashiers and customers, and these compositional changes could be an alternative mechanism behind the slowdown. In this section I explore the sensitivity of my results to estimating the model at the cashier level, with cashier fixed effects. In Appendix A.1, I similarly explore the sensitivity of the results at the customer level, with customer fixed effects.

Supermarket cashier is a position with high-turnover, and the cashiers present at the beginning of the sample are not that same as those at the end. Thus, I average the transaction-level data to the cashier-week level and examine whether including cashier fixed effects alters

[50] Bertrand et al. (2004) discuss issues with estimating difference-in-differences regressions, and find that when more than two periods of data are used, there is a potential for a large number of dependent observations within each cross-sectional unit. One of the solutions they test and recommend is to collapse the data until the dependence issue disappears.

the results. The event study model at the cashier-week level is as follows:

$$Y_{csjw} = \sum_{l=-24}^{24} \beta_l D_{l,jw} + \beta_x X_{csjw} + \alpha_{csj} + \delta_w + \epsilon_{csjw} \tag{2.3}$$

where Equation 2.3 uses average data for cashier c at store s, jurisdiction j, and week-of-sample w. Importantly, the inclusion of cashier fixed effects, α_{csj}, means the β_l coefficients in Equation 2.3 measure the policy effects *within cashiers* over time.

Figure 2.11 presents the cashier-week event study results from estimating Equation 2.3, where I display the $\hat{\beta}_l$ point estimates and standard errors graphically. The outcome variable, Y_{csjw}, is the logged average transaction duration for cashier c at store s, in jurisdiction j and week-of-sample w. In addition to cashier and week-of-sample fixed effects, I control for the average number of items scanned and amount spent per transaction for cashier c in week-of-sample w, as well as the types of items purchased. Additionally, I control for the experience of cashiers, using indicator variables for the number of weeks cashier c had worked the 1:00-4:00pm shift in store s and week-of-sample w. I drop cashiers who are in the sample fewer than 18 weeks (or roughly 4 months), in order to have cashiers that are in the sample long enough to experience learning. This gives me a total of 1,914 cashiers across the 53 store. On average, 36 cashiers work the 1:00-4:00pm weekend shift per store over the 3.5 years of the sample, with a minimum of 19 cashiers and a max of 52 cashiers per store. The median number of weeks worked by cashiers during the sample is 49 out of 177.

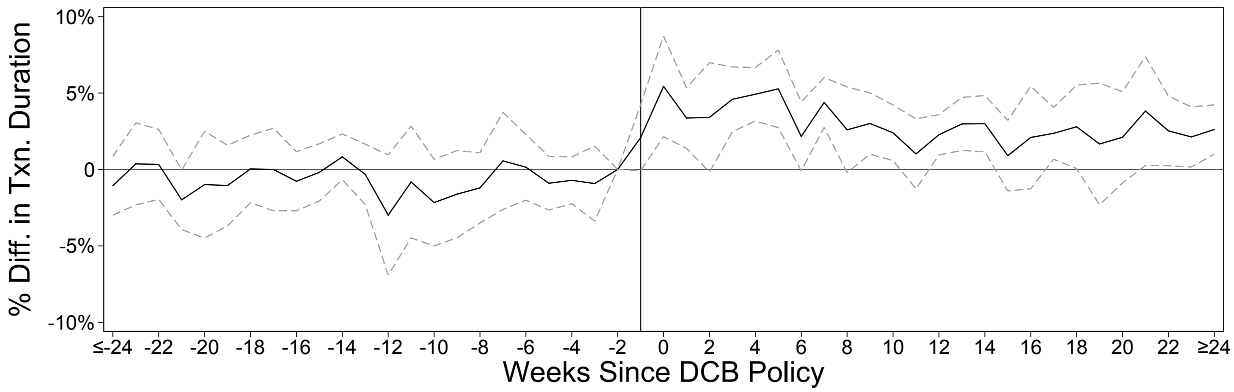
Figure 2.11 presents a slightly different pattern than the store-week analysis in Figure 2.3. First, at the cashier-week level I find that the slowdown in transaction duration began a week or two before the policy.[51] When interviewed, store managers explained that they took measures to prepare their stores for the policy change in the weeks before implementation. In particular, cashiers were asked to start reminding customers of the upcoming policy change. Second, I find that the initial slowdown during the first week of the policy is greater at the cashier level than at the store level. In Figure 2.11, $\hat{\beta}_0 = 0.054$, while in Figure 2.3b, $\hat{\beta}_0 = 0.040$. Third, there is stronger evidence of learning at the cashier level than at the store level, with the post-policy $\hat{\beta}_l$ coefficients diminishing in size over time ($\hat{\beta}_{24} = 0.026$). Using Wald tests to compare the coefficients, I can reject that all $\hat{\beta}_l$ coefficients in the post-policy period are the same as one another at a 1% significance level, and I can reject that $\hat{\beta}_0 = \hat{\beta}_{24}$ at the 10% significance level.

Cashier Learning after DCB Policies vs. Learning after Starting New Shift

To further explore the extent of cashier learning, I examine how cashier learning after DCB policies compares to cashier learning when a cashier first starts working at a store in the 1:00-4:00pm weekend shift. In particular, I estimate the following model:

[51] In Figure 2.11, the omitted event study dummy is $D_{-2,jw}$ instead of $D_{-1,jw}$, so that the slowdown in the week before the policy is clearly visible.

Figure 2.11: Effect of DCB Policies on Transaction Duration (*Cashier-Week Averages*)



Note: Figure presents the full specification of event study Equation 2.3, with cashier and week-of-sample fixed effects and control variables for the average number of items scanned per transaction, average amount spent per transaction, the types of items purchased, and the weeks of experience of cashier c in store s, jurisdiction j, and week-of-sample w. The dependent variable is logged average transaction duration for cashier c in store s, jurisdiction j, and week-of-sample w, measured in minutes. Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample.

For $Treat_{csj} = 0$:

$$Y_{csjw} = \sum_{e=1}^{177} \eta_e E_{e,csjw} + \beta_x X_{csjw} + \alpha_{csj} + \delta_w + \epsilon_{csjw} \tag{2.4}$$

For $Treat_{csj} = 1$:

$$Y_{csjw} = \sum_{e=1}^{177} \eta_e E_{e,csjw} + \sum_{l=-24}^{24} \beta_l D_{l,jw} + \beta_x X_{csjw} + \alpha_{csj} + \delta_w + \epsilon_{csjw} \tag{2.5}$$

where Y_{csjw} is the logged average transaction duration for cashier c in store s, jurisdiction j and in week w, α_{csj} is a vector of cashier fixed effects and δ_w is a vector of week-of-sample fixed effects. $E_{e,csjw}$ is a dummy variable equaling one if cashier c appears in the sample in week w for the eth time (i.e., $E_{1,csjw} = 1$ for all weeks in which cashiers appear in the sample for the first time). $D_{l,jw}$ again is a dummy variable equal to 1 if jurisdiction j in week w enacted a DCB policy l weeks ago, with $l = 0$ denoting the week of implementation. $Treat_{csj}$ is a dummy variable equal to 1 if cashier c is in one of the 33 stores treated. The first week the cashiers are in the sample ($e = 1$) and the first week of the DCB policies ($l = 0$) are the omitted dummies.

Plotting the first twenty-four $\hat{\eta}_e$ estimates alongside the post-policy $\hat{\beta}_l$ estimates allows me to compare the learning curve from starting to work in the 1:00-4:00pm weekend shift versus the learning curve from working with a DCB policy in place. For graphing purposes, I estimate the model separately for treated and control stores, however the results do not change when I pool the sample.

Figure 2.12 presents the results. On the left side of the graph, I plot the $\hat{\eta}_e$ estimates. The estimates for cashiers at treated stores are depicted with red circles and the estimates for cashiers at the control stores are depicted with blue hollow circles. I find that $\hat{\eta}_2 = -0.028$ at the treated stores, which means that between the first and second week cashiers work the 1:00-4:00pm weekend shift, they become 2.8% faster in completing a transaction. $\hat{\eta}_3 = -0.039$ means cashier checkout speed is 3.9% faster in the third week of work than the first week. The quickening of checkout duration continues at a diminishing rate. By week 24 of working the 1:00-4:00pm shift, the reduction in speed ceases and cashiers remain approximately 10% faster than their first week. This pattern holds for cashiers at the control stores as well.

I fit these coefficients into the conventional from of a learning curve (Alchian, 1963; Argote and Epple, 1990):

$$T_N = T_1 * N^b \tag{2.6}$$

where T_N is transaction duration for the Nth week of working the 1:00-4:00pm shift, T_1 is transaction duration in the first week, and $b = \frac{ln(LearnRate)}{ln(2)}$ is the slope of the learning curve. I estimate $T_N = 1.904 * N^{-0.033}$ which corresponds to a learning curve rate of 97.7%. This means that transactions in the second week take 97.7% the time of the first week, and transactions in the fourth week take 97.7% of the second week and so on.[52]

On the right side of Figure 2.12, I plot the post-policy $\hat{\beta}_l$ estimates (i.e., $\hat{\beta}_1$ to $\hat{\beta}_{24}$). I find evidence of changes in checkout speed from the first week of DCB policies (the omitted $\hat{\beta}_0$) to the subsequent policy weeks in that most of the $\hat{\beta}_l$ estimates are negative. Moreover, after policy week ten, where $\hat{\beta}_{10} = -0.29$, the majority of $\hat{\beta}_l$ coefficients are statistically less than zero at the 10% significance level. Note, I am not estimating the effect of the policy compared to the pre-period as I did in the event study estimations in Figure 2.11. Instead, I am estimating the change from the first week of the policy.[53] Fitting these coefficients into a learning curve (Equation 2.6) reveals $T_N = 1.879 * N^{-0.008}$, where T_N is now transaction duration in the Nth week of the policy. This corresponds to a learning curve rate of 99.4%.

Comparing Figure 2.11 and the halves of Figure 2.12 suggests that cashiers do learn and get faster after DCB policies; however, this learning curve is much shallower than the learning curve when starting a new shift. Moreover, cashier learning of 2-3% does not completely offset the 5% initial within cashier slowdown from the policies. Thus the reduction in productivity from DCB policies persists even after cashiers learn and adapt to the change. This exercise also provides a framework for evaluating the magnitude of checkout productivity slowdown from the DCB policies. A 3% slowdown in checkout speed would be similar to switching a

[52]In comparison, the 1-year death rate for hospitals performing heart transplants follows a 79% learning curve and the production rate of aircrafts follows a 80% learning curve (Heizer and Render, 2013).

[53]This is similar to re-centering the estimates in Figure 2.11 so that β_0 lies on the x-axis instead of β_{-2}.

Figure 2.12: Learning Curve: Starting a New Shift vs. DCB Policies (*Cashier-Week Averages*)

Note: Figure presents the results from equations 2.4 and 2.5, with cashier and week-of-sample fixed effects and control variables for the average number of items scanned per transaction, average amount spent per transaction, and the types of items purchased for cashier c in store s, jurisdiction j, and week-of-sample w. The dependent variable is logged average transaction duration for cashier c in store s, jurisdiction j, and week-of-sample w, measured in minutes. The η_e estimates (for cashiers learning to work a new shift) are plotted on the left side of the graph. The post-policy $\hat{\beta}_l$ estimates (for cashiers learning after the policy change) are plotted on the right side of the graph. The estimates for cashiers at treated stores are depicted with red circles and the estimates for cashiers at the control stores are depicted with blue hollow circles. Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample.

cashier who has worked 20 weeks with a cashier that has only worked 10 weeks, or switching a cashier that has worked 3 weeks with one that is just starting.

Within Customer Effects

I also explore the sensitivity of the results at the customer level. The important take-away from the customer level analysis, presented in Appendix A.1, is that including customer fixed effects does not alter the main results in Figure 2.3. I find that DCB policies lead to sharp and persistent increases in checkout duration *within customer*, which mean the checkout slowdown in the store level analysis is not driven by changes in the composition of customers. I also find that the heterogeneity results in Figure 2.8—where larger transactions purchasing paper bags experience greater slowdowns—replicate at the customer level. This is reassuring because, at the store-week level, splitting the transactions by whether a paper bag was purchased in the post-period meant that the treated customers in the pre-period were not necessarily the same as the treated customers in the post-period. With the customer level data, I split the customers at treated stores in four groups by whether they ever buy paper bags and by transaction size. I find that none of the treated household groups differ from the control households in the pre-period. Yet after the policy, treated customers with larger transactions and those that choose paper bags have longer transaction durations than control customers.

2.6 Robustness and External Validity: Evidence from Supplementary Data

In the following subsections I explore supplementary datasets to test the robustness of the results above as well as their external validity. In Section 2.6, I compare the effects of DCB policies on transaction duration using scanner data versus using observational data collected in-store. In Section 2.6, I estimate the effects of DCB policies at an alternative supermarket chain, to investigate whether the checkout slowdowns are a general phenomenon or unique to the retail chain in the main analysis. In Section 2.6, I analyze whether slowdowns occur under a different type of policy (i.e., a bag tax), in a different state and time period.

Robustness Analysis 1: Scanner Data vs. In-store Data

While the supermarket scanner dataset is rich along several dimensions, it is missing three key variables: i) the presence of baggers at checkout, ii) the types and number of bags customers use before and after the policy change, both purchased and brought from home, and iii) the amount of downtime, if any, between transactions. To address these data limitations, I designed a follow-up field experiment—taking advantage a DCB policy implemented in Contra Costa County California on January 1, 2014. I made bi-weekly visits to three supermarkets—of the same retail supermarket chain as the scanner data—to collect data through direct observation of checkout transactions. Enumerators, stationed near full-service registers, collected information on the number and types of bags used, the presence of a bagger, the duration of each transaction, and basic customer demographic information such as gender and race of the person paying.[54] These visits were made over five months—one month before (December) and four months after (January-April) the policy change in Contra Costa County. Each visit lasted 1-2 hours and was made on either a Saturday or Sunday between 11:00am and 7:00pm. I also obtain the scanner data for the same dates and hours as the

[54]Observations were made only at full-service registers, and not express or self-checkout registers.

in-store visits. In this subsection, I use the in-store data to examine the effects of DCB policies controlling for variables that cannot be measured with the scanner data.

The first store, which I refer to as the *treated* store, is in Richmond, a city that implemented a DCB policy during my sample period. The second store, which I refer to as the *prior-policy* store, is in Berkeley, a city that adopted a DCB policy in January 1, 2013, exactly one year before the Richmond policy. The third store, which I refer to as the *no-policy* store, is in Concord, a city that has yet to adopt a DCB policy. The two control cities were chosen to match Richmond with respect to average demographic characteristics.[55]

How do the in-store and scanner datasets compare along the variable of interest—transaction duration? In particular, I am concerned that my measure of transaction duration in the scanner dataset may overestimate the actual transaction duration because of the potential downtime in between transactions that is missing in the scanner dataset. In Table 2.5, I compare the average transaction duration, measured in minutes, for the in-store and scanner datasets. For the full sample of transaction, the average transaction duration in the in-store dataset is 0.119 minutes shorter than in the scanner dataset, which translates to roughly 7.14 seconds. Thus I do find that the scanner data misses a portion of downtime in between transactions. However, the worry is not that this difference occurs but that it happens differentially at stores with and without DCB policies. Thus I compare the average transaction duration between in-store and scanner for stores with and without DCB policies. Reassuringly, I find similar differences between in-store and scanner datasets when splitting the sample by policy treatment.

I next examine the effects of DCB policies on transaction duration at full-service registers using the in-store observational data. I estimate the following event study model:

$$Y_{tsjdm} = \sum_{l=-1}^{3} \beta_l D_{l,jm} + \beta_x X_{tsjdm} + \theta_{sj} + \delta_{dm} + \epsilon_{tsjdm} \tag{2.7}$$

where Y_{tsjdm} is the outcome variable for transaction t in store s on date d in month m, $D_{l,jm}$ is the set of monthly event study dummies, X_{tsjdm} are control variables, θ_{sj} are store fixed effects, and δ_{dm} are date fixed effects.

Figure 2.13 presents the event study results, with the outcome variable being either logged transaction duration (panels a and b) or the probability of having a bagger (panel c). I juxtapose the results of using in-store data (panel a) with the results using scanner data (panel b). The scanner data comes from the full-service registers at the same three stores and on the same dates as the in-store data.[56] In both panels (a) and (b), I observe that the DCB policies led to an increase in checkout duration. Reassuringly, the $\hat{\beta}_l$ coefficients using the

[55] I designed the in-store data collection to answer multiple questions about the effects of DCB policies. In Taylor and Villas-Boas (2016b), the in-store data were used to measure how checkout bag choices change when DCB policies go into effect. Please see Appendix A.3 for a more detailed description of the variables in the in-store data.

[56] With the observational data, X_{tsjdm} contains indicators for the gender and race of the person paying, whether there was a checkout interruption, and register fixed effects. With the scanner data, X_{tsjdm} contains the number of items scanned, the amount spent, and register, hour, and cashier fixed effects.

Table 2.5: Average Transaction Duration (*In-store vs. Scanner Data*)

	In-store Data	Scanner Data	Difference
Full Sample Mean	1.718	1.837	-0.119***
SD	(1.114)	(1.234)	
N	1,692	34,028	
Stores with DCB Policy	1.756	1.910	-0.154***
SD	(1.144)	(1.252)	
N	934	17,562	
Stores without DCB Policy	1.670	1.759	-0.089**
SD	(1.073)	(1.210)	
N	758	16,466	

Note: Standard deviations in parentheses. Asterisks indicate the following: * p < 0.10, ** p <0.05, *** p <0.01. *Source*: Author's calculations from observational data collected in-store and from the scanner data corresponding to the same days and stores as the observational data.

in-store data are comparable in size to the coefficients using scanner data.[57] These results are also consistent with the main event study results in Section 2.4, using the scanner data from 53 stores, in that I find a significant and persistent slowdown in transaction duration.[58]

In panel (c), using the in-store data, I find that the probability of a transaction having the assistance of a bagger temporarily decreases after the DCB policies go into effect. This could occur for several reasons. On one hand, if the same number of baggers are present after the policy as before but their presence is required for a longer period of time per transaction, they can not float to as many transactions as before the policy. Alternatively, stores may decide to use fewer baggers when their comparative advantage in packing the thin plastic bags becomes extraneous.

Matching In-Store to Scanner Data

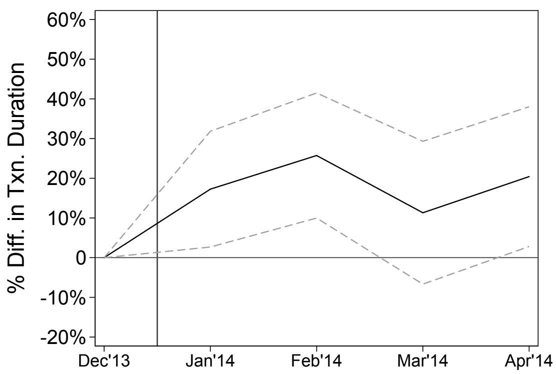
Since I have scanner and in-store data for the same days, hours and stores, I next match the scanner data transactions to their corresponding in-store data transactions. This is a challenging task as transactions that appear as one to the in-store observer may be rung up

[57]In Appendix A.4, I estimate a difference-in-differences model using both the scanner and in-store datasets and find similar results as these event studies.

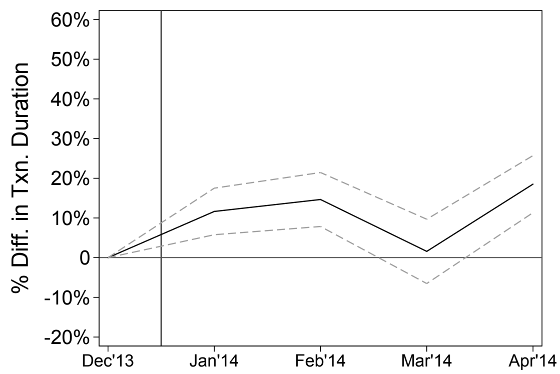
[58]However, the $\hat{\beta}_l$ coefficients are much larger using the three store sample. In Figure 2.13a, $\hat{\beta}_0 = 0.173$, which is 4 times larger than what was estimated in Figure 2.3b, where $\hat{\beta}_0 = 0.040$. This difference may be driven by the shorter sample period of the three store data, especially in the pre-policy period (i.e., without multiple years of data and only one treated store in the sample, I am unable to fully control for seasonality and confounding factors).

Figure 2.13: Effect of DCB Policies on Transaction Duration and Bagger Presence (*In-store vs. Scanner Data*)

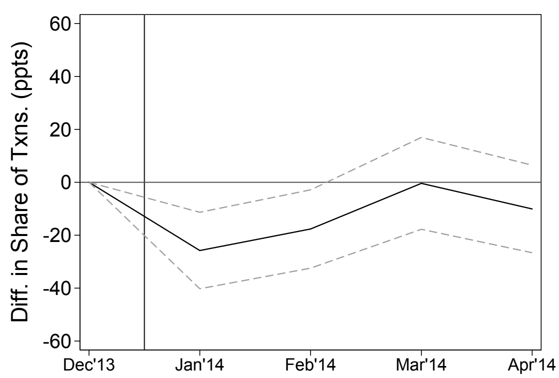
(a) Full-service Registers (*In-store Data*)



(b) Full-service Registers (*Scanner Data*)



(c) Bagger Presence (*In-store Data*)



Note: The figure panels display the $\hat{\beta}_l$ coefficient estimates from the full specification of event study Equation 2.7. The dependent variable in panels (a) and (b) is logged average transaction duration, measured in minutes, of transaction t in store s and day-of-sample d. The dependent variable in panel (c) is an indicator equal to 1 if transaction t had a bagger present. Panels (a) and (c) use observational data collected in store while panel (b) uses scanner data. This analysis includes only transactions occurring at full-service registers, and not express or self-checkout registers. With the observational data in panel (a), the control variables include indicators for the gender and race of the person paying, whether there was a checkout interruption, and the register number. With the scanner data in panel (b), the control variables include the number of items scanned, the amount spent, the register number, and hour and cashier fixed effects. Upper and lower 90% confidence intervals are depicted in gray. Standard errors are calculated using error clustering at the store-day level.

as two transactions in the scanner data, and visa versa.[59] Thus far, in-store transactions from December 2013 (pre-policy) and January 2014 (post-policy) have been matched to the scanner data, which is roughly 41% of the transactions in the in-store sample.

From this matched data, I can calculate that the average plastic bag holds 3.805 items, the average paper bag holds 9.087 items, and the average brought reusable bag holds 8.744 items. Comparing transactions of similar size at the treated store, on average plastic bag transactions spend 7.244 seconds per item, paper transactions spend 8.475 seconds per item, and reusable transactions spend 7.619 seconds per item. While I can reject that paper and plastic bag transactions take the same amount of time per item at the 5% significance level, I cannot reject that reusable and plastic bag transactions take the same amount of time per item. This suggests paper bags are a slower technology, taking roughly a second more per item than plastic and reusable bags.

Do Transactions Shift into Different Hours of the Day?

Using the scanner data from the three store sample, I can also explore whether transactions shift into the less busy hours around the peak hours. For my main analysis (Section 2.4), I only observe transactions that occur during the hours of 1:00pm to 4:00pm, and thus, I cannot test whether transactions shift into less busy hours of the day. In the scanner data from the three store analysis, while I have a much smaller sample of stores and days, I observe all transactions made between 11:00am and 7:00pm.

With these data, I estimate the difference-in-difference model in Equation 2.1 for each hour between 11:00am and 7:00pm, with the outcome variable being (a) the number of transaction completed in each hour (or groups of hours) and (b) the average number of registers open in each hour (or groups of hours). Figure 2.14 plots the $\hat{\beta}_D$ coefficients. In panel (a) I find evidence that while the number of transactions decreases during the peak hours of 1:00pm to 4:00pm, the number of transactions remains the same or increases in the less busy hours surrounding the peak hour.[60] This provides suggestive evidence that some transactions lost during peak hours, due to the slowdowns from DCB policies, are made up in different hours of the day. However, when I estimate the model summed to the store-by-date level (denoted as *All Day* in Figure 2.14), I still find a decrease in the number of transactions processed, though it is no longer statistically significant.

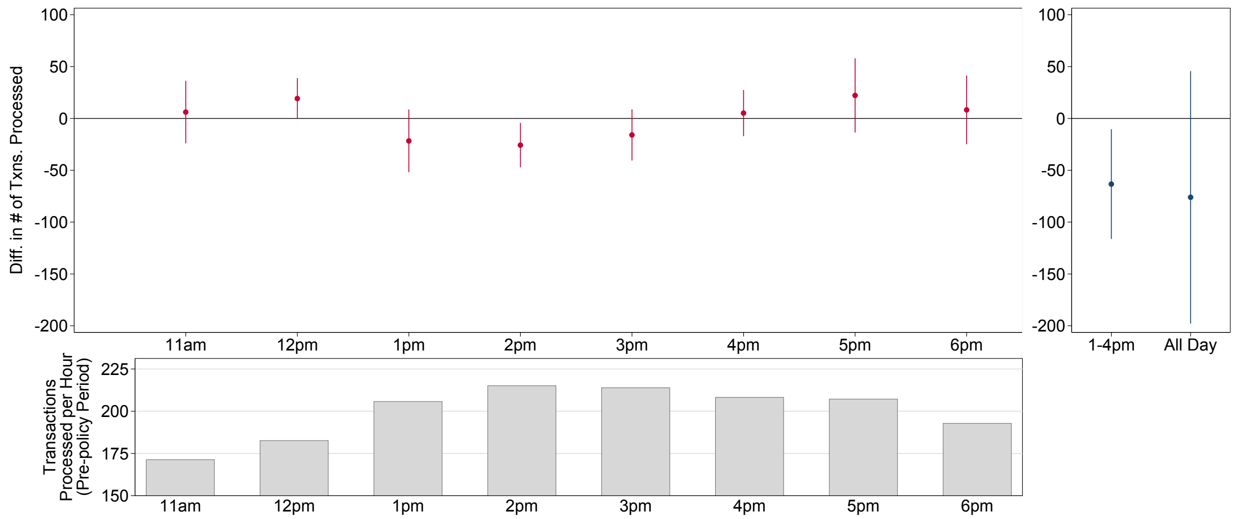
In panel (b), I examine whether treated stores alter the number of registers open by hour of the day. Similar to Figure 2.10, during the peak 1:00-4:00pm hours, I do not find evidence that treated stores change the number of registers open. During the less-busy hours of 11:00am-1:00pm and 5:00-7:00pm, which have fewer registers open on average in the pre-policy period, the $\hat{\beta}_D$ estimates are slightly larger. However, none of the estimates are ever statistically different from zero, potentially due to the small sample size.

[59]This can occur when a customer splits their purchase into smaller purchases or when a large group of customers move through the line together.

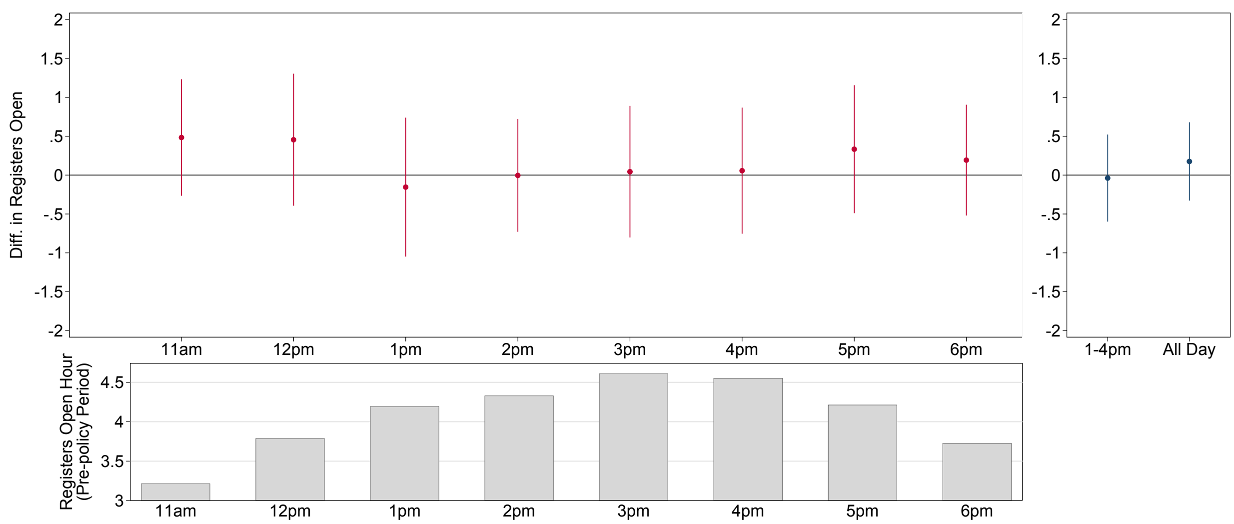
[60]For instance, $\hat{\beta}_D = 19.218$ for the 12:00pm hour, $\hat{\beta}_D = -25.734$ for the 2:00pm hour, $\hat{\beta}_D = 22.225$ for the 5:00pm hour.

Figure 2.14: Effect of DCB Policies by Hour and by Shift (*Scanner Data from Three-Store Sample*)

(a) Number of Transactions Processed by Hour/Shift



(b) Number of Registers Open by Hour/Shift



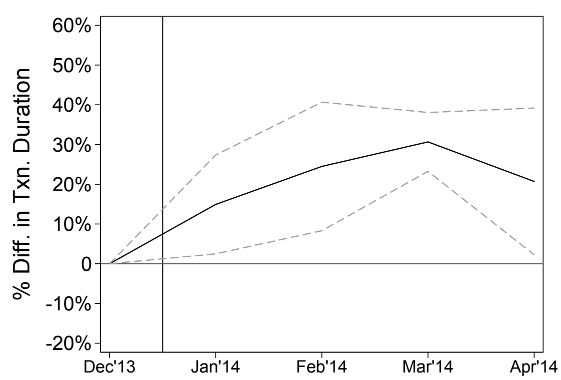
Note: The figure panels display the $\hat{\beta}_D$ coefficients estimates from the difference-in-differences Equation 2.2. The dependent variable in panel (a) is the number of transaction completed (by hour or by shift) in store s and week w. The dependent variable in panel (b) is the number of registers open (by hour or by shift) in store s and week w. Upper and lower 90% confidence intervals using robust standard errors are depicted in gray.

The panels in Figure 2.14 provide suggestive evidence that DCB policies cause some transactions to shift into less congested hours of the day. Moreover, stores may react to DCB policies by opening more registers in hours shouldering the peak hours, when they are not constrained by register capacity.

Robustness Analysis 2: Discount Chain

To explore whether this phenomenon is unique to the supermarket chain in my main analysis, I use supplementary data from a markedly different retail grocery chain. In additional to collecting observational data at the chain used throughout this paper, I collected data at a discount chain within the same three treated and control California cities as describe in Section 2.6. While the main chain is a large national chain, offering high and low prices in many products, the discount chain is a regional chain, offering name-brand products at closeout prices. Not only do these chains attract a different clientele within the same cities,[61] their management also chose different responses to the same DCB policy. The national chain chose to charge the minimum required five cents per paper bag and the discount chain chose to charge ten cents per paper bag and introduced a 15-cent thick-plastic reusable bag. By running the same analysis on each of these chains, I am able to compare the effects of DCB polices across retail settings.

Figure 2.15: Effect of DCB Policies on Transaction Duration (*Discount Chain In-store Data*)



Note: The figure displays the $\hat{\beta}_l$ coefficient estimates from the full specification of event study Equation 2.7. The dependent variable is logged average transaction duration, measured in minutes, of transaction t in store s and day-of-sample d. This figure uses observational data collected in store at full-service registers. The control variables include indicators for the gender and race of the person paying, whether there was a checkout interruption, and the register number. Upper and lower 90% confidence intervals are depicted in gray. Standard errors are calculated using error clustering at the store-day level.

[61]The discount chain has a 15 percentage point greater share of minority customers than the national chain.

I replicate the analysis in Section 2.6 with the observational data from the discount chain.[62] Comparing the event study results in Figure 2.13a and Figure 2.15, shows that DCB policies lead to increases in transaction duration, even at stores in a different retail chain. In fact, the percent slowdown in transaction duration at the discount chain is even larger than at the national chain. While this comparison is suggestive and not causal, policymakers might be concerned if DCB policies affect low-income shoppers more so than wealthier shoppers, or if DCB policies affect regional stores more so than national stores.

Robustness Analysis 3: Washington DC Bag Tax

Are the supermarket checkout slowdowns I estimate above unique to California DCB policies, where plastic bags are banned and paper bags require a fee, or are they characteristic of other DCB policies passed in the U.S.? To answer this question, I use scanner data from the same supermarket retailer as above, but for stores in the District of Columbia (DC) metropolitan area. While California has favored using plastic bag bans and paper bag fees because a state law temporarily prohibited the taxing of plastic bags,[16] local governments in other states have had more flexibility in their policy tool options.[63] On January 1, 2010, DC enacted a bag tax, requiring all stores that sell food items to charge a 5-cent tax per plastic or paper bag issued. Using scanner data from DC, I examine the effects of a different type of DCB policy in a different region, in order to learn about the generalizability of my results.

The DC scanner dataset covers 4 months in the pre-tax period (Dec. 2008, Jan. 2009, Feb. 2009, Dec. 2009) and 5 months in the post-tax period (Jan. 2010, Feb. 2010 Dec. 2010, Jan. 2011 and Feb. 2011). The sample includes all transactions during the peak hours of 3:00-5:00pm on weekends and 5:00-7:00pm on weekdays during these months. I select six stores within DC that are open without interruption between December 2008 and January 2011. I select an additional 12 stores, within a 20 mile radius of DC, that best match the DC stores in terms of building age and size and census block-group characteristics. This gives me six treated and twelve control stores.[64]

[62] Please see Appendix A.3 for a more detailed description of the variables in the in-store data at the discount chain and Appendix A.4 for the results of a difference-in-differences analysis using these discount chain data.

[63] Conversely, some states (including Arizona, Idaho, Michigan, and Missouri) have passed laws that ban local governments from banning or taxing plastic bags ("State Plastic and Paper Bag Legistlation; Fees, Taxes and Bans | Recycling and Reuse." *National Conference of State Legislatures.* Nov. 11, 2016. Online, *accessed Dec. 18, 2016*).

[64] Appendix Table A.7 presents the summary statistics for the treated and control stores in the pre-policy period. I find that treated and control stores are balanced across most store and demographic characteristics. With respect to transaction level characteristics, in addition to presenting the averages from the entire sample of transactions, I split the transactions in half by transaction size and present the averages for the smallest (less than 8 items scanned) and largest (8 or more items scanned) transactions. I drop transactions that occur in self-checkout registers because only 4 of the 18 stores have self-checkout during my sample period. Overall, I find that transactions in the treated DC stores during the pre-policy period take slightly longer, but have roughly the same size and expenditures as transactions in the control stores. At treated stores, the average transaction takes approximately 2 minutes to complete, comprises of 12 items scanned, and costs

I estimate the effect of the DC bag tax on transaction duration with data average to the store-week level and the following event study model:

$$Y_{sjw} = \sum_{l=-5}^{8} \beta_l D_{l,jw} + \beta_x X_{sjw} + \theta_{sj} + \chi_w + \epsilon_{sjw} \tag{2.8}$$

where Y_{sjw} is the logged transaction duration in store s, jurisdiction j, and week-of-sample w, $D_{l,jw}$ are indicators for transactions at the treated stores in DC during the weeks before and after the bag tax, X_{sjw} is the set of control variables, θ_{sj} are store fixed effects, and δ_w are week-of-sample fixed effects. $D_{-5,jw}$ equals one for all weeks between December 2008 and February 2009 (i.e., a year before policy implementation). Similarly, $D_{8,jw}$ equals one for all weeks between December 2010 and February 2011 (i.e., a year after policy implementation). The week prior to implementation ($l = -1$) is the omitted category.

Figure 2.16 plots the results. Given the estimates I find from the California DCB policies—where the effect of DCB policies on transaction duration depends on the size of the transaction and whether or not a customer chose to pay the bag fee—I estimate the model for the entire sample (panels a and b) and by transaction size and disposable bag use (panels c to f). In panel (a) the outcome variable is in logs and in all other panels it is in levels.

I find strong evidence that the DC bag tax leads to slower transactions. In panel (a), during the first week of the policy transactions take 9.0% longer to complete. In panel (b), the 9.0% slowdown translates to 0.183 minutes more per transaction. This slowdown lessens substantially over time, with $\hat{\beta}_8 = 0.088$ (a year after the policy) nearly half the magnitude of $\hat{\beta}_1 = 0.183$. Unlike the California DCB policies shown in Figure 2.3, I can reject that $\hat{\beta}_1 = \hat{\beta}_8$.[65] Thus, even though the effects of the DC bag tax on transaction duration do not fully dissipate after a year, I find stronger evidence of learning than under the California bag bans.

Next I examine heterogeneity by transaction size and bag choice. For the smallest transactions that do not pay the bag fee (panel c), I find a small but not statistically significant slowdown in transaction duration after policy implementation. The opposite is true for small transactions that do pay the fee (panel d), with $\hat{\beta}_0 = 0.110$ minutes and $\hat{\beta}_8 = 0.092$ minutes. For the largest transactions (panels e and f), I find statistically significant slowdowns for both the transactions that pay the fee and those that do not, and these slowdowns diminish over time. For the larger transactions that do not pay for disposable bags (panel e), the slowdown peaks in the second week of the policy ($\hat{\beta}_0 = 0.0882$, $\hat{\beta}_1 = 0.233$, $\hat{\beta}_8 = 0.124$). For the larger transactions paying the fee (panel f), the slowdown peaks in the first week of the policy ($\hat{\beta}_0 = 0.250$, $\hat{\beta}_1 = 0.218$, $\hat{\beta}_8 = 0.087$). This suggests that the aggregate slowdown (panels a and b) was at first due to the newness of paying the fee (i.e., people were not expecting to pay for bags during the first week of the policy), but the persistent effects of the bag tax on transaction duration result from the alternatives to paying for disposable bags.

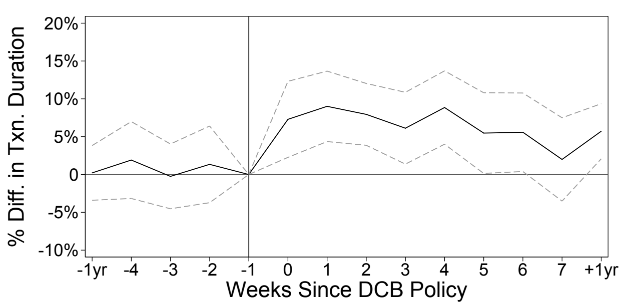
$35.

[65] $F(1, 570) = 8.35$, p-value $= 0.0040$

Figure 2.16: Heterogeneity in Effect of DCB Policies on Transaction Duration, by Transaction Size and DCB Purchase (*DC Data, Store-Week Averages*)

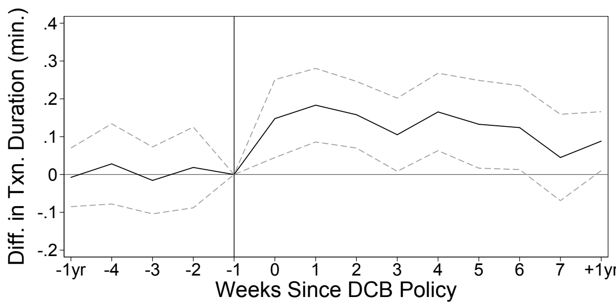
(a) Logged Transaction Duration

Full Sample



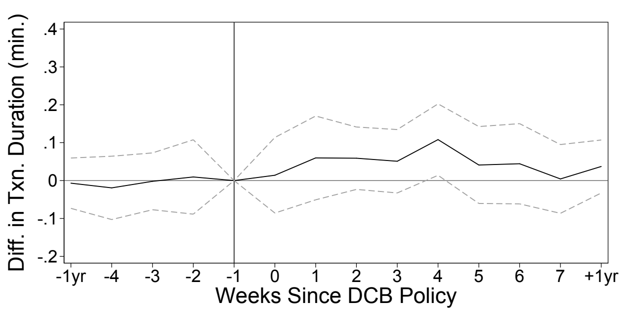
(b) Transaction Duration

Full Sample



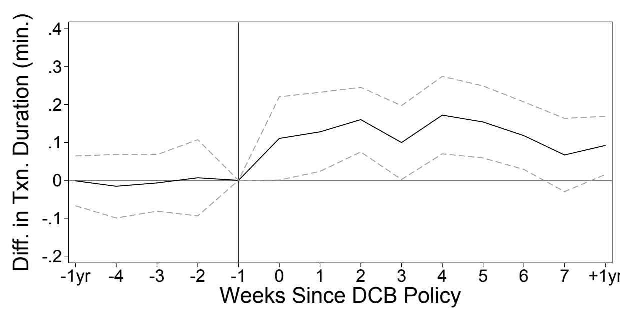
(c) Transaction Duration—Scans < 8

Without paying bag fee



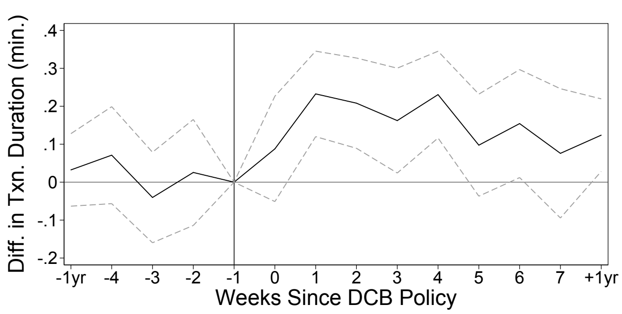
(d) Transaction Duration—Scans < 8

Paid bag fee



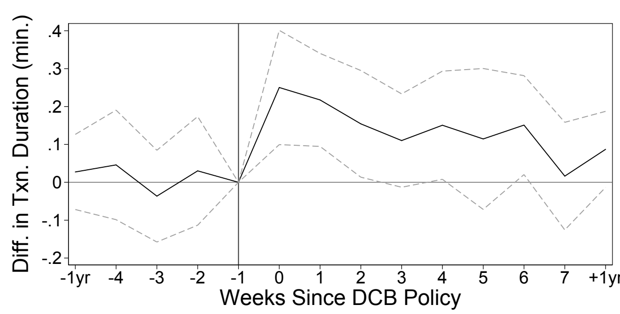
(e) Transaction Duration—Scans ≥ 8

Without paying bag fee



(f) Transaction Duration—Scans ≥ 8

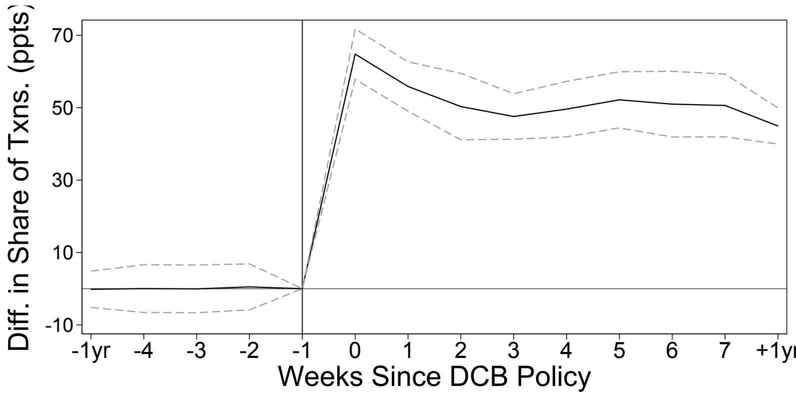
Paid bag fee



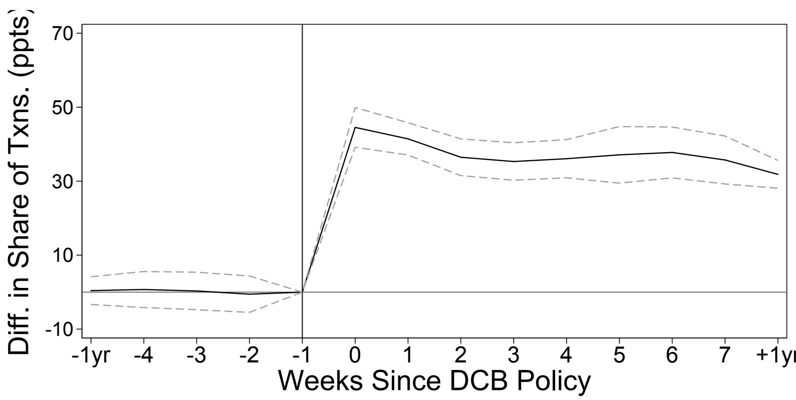
Note: The figures display the $\hat{\beta}_l$ coefficient estimates from the full specification of event study Equation 2.8. The dependent variable is average transaction duration, measured in minutes, in store s and week-of-sample w. The dependent variable is in logs in panel (a) and in levels in all other panels. Upper and lower 95% confidence intervals using robust standard errors are depicted in gray. D_{-5} equals one for all weeks w in December 2008 through February 2009 (i.e., a year before policy implementation). Similarly, D_8 equals one for all weeks in December 2010 through February 2011 (i.e., a year after policy implementation).

Figure 2.17: Effect of DCB Policies on Share of Transactions Paying Bag Fee, by Transaction Size (*DC Data, Store-Week Averages*)

(a) Share Paying Fee—Scans ≥ 8



(b) Share Paying Fee—Scans < 8



Note: The figures display the $\hat{\beta}_l$ coefficient estimates from the full specification of event study Equation 2.8. The dependent variable is the share of transactions paying the bag fee. Upper and lower 95% confidence intervals using robust standard errors are depicted in gray. D_{-5} equals one for all weeks w in December 2008 through February 2009 (i.e., a year before policy implementation). Similarly, D_8 equals one for all weeks in December 2010 through February 2011 (i.e., a year after policy implementation).

In Figure 2.17, I estimate Equation 2.8 with the share of transactions paying the bag fee as the outcome variable, separately for the largest and smallest transactions. Previous research has found that the vast majority of customers paying the DC bag fee chose plastic over paper bags (Homonoff, 2016). I find that 65% of the largest transactions pay for a disposable bag in the first week. This drops to 56% by week 2 and 50% by week 3. A year after the policy, 45% of large transactions pay the fee. For the smallest transactions, 45% pay the fee in week 1 and 32% a year later. Thus it does appear that some shoppers were surprised by the policy in its first week and altered their bag choice behavior in subsequent weeks.

Comparing the event study results for the DC and California policies, I find evidence that the mechanisms behind the slowdown are not the same across policy tools, and that the slowdown is less persistent over time under a bag fee than a bag ban. The results in Figure 2.16 are similar to what I find in Figure 2.8 in one way—I do not find slowdowns due to either policy for the smallest transactions that opt not to pay for DCBs. However, the DC results differ from California in that the largest percent slowdowns a year after the policy do not occur for the larger transactions paying the bag fee. Instead, *both* large and small transactions paying the fee are 0.09 minutes slower than transactions at control stores a year after the policy. Therefore, when plastic bags have a fee, there is a fixed time cost of paying the bag fee which is independent of transaction size. Conversely, when plastic bags are banned and paper bags have the fee, there is also an additive time cost which scales with items purchased.[66] Adding this to the evidence that paper bags are a slower technology than plastic bags (Section 2.6), suggests that bag taxes may have lower time costs than plastic bag bans coupled with paper bag fees.

2.7 Discussion of Broader Impacts

Checkout Congestion, Queue Length, and Customer Wait Time

The results above indicate that DCB policies in California cause persistent 3% increases in the amount of time to checkout at supermarkets. How does this increase in processing time per transaction impact the amount of time customers spend in line waiting to checkout? During peak hours, when checkout transactions occur back-to-back, a customer not only has to wait the extra time for their own transaction, they must also wait the extra time for all the customers ahead of them in line. Even though the scanner data does not measure queue length directly, I can use the scanner data and a simple queuing theory model to approximate how many more people are waiting in line due to DCB policies. First, I quantify the change in the number of transactions completed per store and shift by estimating Equation 2.2 with

[66] Additionally, the DC results differ from California in that the larger transactions not paying the fee (panel e) experience slowdowns of similar magnitude as the larger transactions paying the fee (panel f). Since the DC policy predates the California policies in my sample by at least two years, this may be due to DC cashiers and customers having less experience with reusable bags when the policy went into effect.

the outcome variable being the average number of transactions completed per three-hour weekend shift. If we assume that the arrival process to checkout does not change as a result of the DCB policies (an assumption I can relax later), then a decrease in the number of transactions processed would mean these "missing" transactions are still waiting in line to be processed. I then calculate the average increase in the number of transactions waiting to be processed as $\frac{|\hat{\beta}_D|}{2}$.[67]

The top panel of Table 2.4 present the $\hat{\beta}_D$ coefficients using the scanner data from the 53 stores across California, estimated in both levels and logs. In column (2), I find that stores process 18.2261 fewer transactions per three-hour shift when DCB policies go into effect, which equals a 3.0% decrease in the number of transactions completed per shift.[68,69] In column (2) I use transaction duration again as the outcome variable instead of transactions per shift. Corroborating the event study results in Figure 2.3, I find that DCB policies lead to a 3.3% increase in transaction duration. Comparing columns (1) and (2), the 3% increase in transaction durations translates to a 3% decrease in the number of transactions processed, which means stores are not absorbing any of the slowdown from DCB policies during these peak-hour shifts.

I report the $\frac{|\hat{\beta}_D|}{2}$ estimate in the first row of the bottom half of Table 2.4 and then use it to calculate the additional number of customers in line *per register* either: (i) given the average number of registers open in the post-policy period, or (ii) if all existing registers were open. In column (2), with an average 8.931 registers open in the post-policy period, the DCB policies cause a 1.020 transaction increase in queue length *per register open*. If instead all existing registers were open—the average of which is 10.491 registers—DCB policies would add an additional 0.869 transactions per queue. Therefore, the slowdown in transaction duration from DCB policies causes each checkout queue to be approximately 1 customer longer on average.

These are upper bounds for changes in queue length. At the other extreme, queue length may not change if customers not served during the 1:00-4:00pm shift decide to shop at a different (and potentially less crowded) time of the day/week, or, at a different store altogether. Not only is grocery shopping less convenient under DCB policies, restaurants and food-away-from-home establishments are exempted from these policies and thus can still

[67] I divide $|\hat{\beta}_D|$ by 2 to get the average change in customers standing in line per store at any given moment during a 1:00-4:00pm shift, conservatively assuming the increase in line length is zero at the beginning of the shift and grows linearly to $|\hat{\beta}_D|$ by the end of the shift. This is conservative because the peak shopping hours extend before and after 1:00-4:00pm on weekends, making it likely that an increase in line length would have started before 1:00pm.

[68] In Appendix Table A.8, I explore whether the result in Table 2.4 column (2) varies by store characteristics. I find evidence that the decrease in transactions completed due to the policy change is greater for stores in census blocks-groups with a higher median income, a lower share of Asian residents, and a higher share of Black residents. This finding is consistent with the results in Appendix Table A.3—which showed paper bag use is positively correlated with income and negatively correlated with the Asian population share—and the result in Figure 2.8—which showed greater slowdowns for transactions choosing paper bags.

[69] In Appendix Figure A.2, I estimate this as an event study using Equation 2.1 and find similar results.

offer disposable plastic bags to their customers. Since dining out and grocery shopping are substitute goods, and DCB policies effectively raise the cost of grocery shopping relative to dining out (whether in the form of a convenience cost or the actual cost of bags), some of the 18 customers not served during the shift may have chosen to purchase food elsewhere. In future work I will empirically test whether DCB policies shift customers away from grocery shopping towards eating out.

Interpreting the Time Cost of Checkout Congestion

How would customers fare if each checkout line is 1 customer longer during peak hours? The median transaction in my sample in 2011 was approximately 2 minutes. An industry white paper finds that half of grocery shopping transactions in the U.S. occur during peak hours (Goodman, 2008), where a peak hour is defined as a time wherein more than 3 million people shop during that hour of the week.[70] Thus, if DCB policies cause the average queue to increase by 1 transaction during peak hours and half of all transactions occur during peak hours, this would translate to an average additional 1.09 minutes of wait and processing time ($1.09 = 2*1.03*0.5+2*0.03$). For busy customers, this is not a negligible wait time. Given the average grocery shopping trip on weekends lasts 44 minutes (Hamrick et al., 2011), a 1.09 minute longer wait translates to shoppers spending 2.5% more time in store per trip. An industry survey found that the average wait time in grocery shopping lines in 25 major cities was 4 minutes, meaning DCB policies lead to a 27% increase in grocery shopping checkout wait time.[71]

Using half the average California hourly wage as the value of time—since grocery shopping often occurs during non-work hours when the opportunity cost of time is low[72]—1.09 minutes is worth \$0.24. Aggregating to the state level, the total time cost of a statewide DCB policy for the 28 million Californian adults would be as much as 25.8 million hours annually (≈\$343 million).[73] To get a sense of the magnitude of this time cost, I turn to the literature on the time savings of policies affecting traffic congestion. Anderson (2014) finds that Los Angeles public transit saves 114 million hours of traffic congestion delay each year. Foreman (2013) finds that a bridge toll price change in the San Francisco Bay Area saved 210,000 hours

[70] "Grocery Shopping: Who, Where, and When." *Time Use Institute.* Oct. 2008. Online, *accessed Sep. 9, 2016.*

[71] "Justice—Wait for It—on the Checkout Line." *Wall Street Journal.* Aug. 19, 2009. Online, *accessed May 30, 2016.*

[72] I use half the hourly wage because it is a generally accepted figure for the value of non-work time (Small, 1992; Small and Verhoef, 2007). Half the average California hourly wage is \$13.29 ("May 2015 State Occupational Employment and Wage Estimates, California." *Bureau of Labor Statistics, U.S. Dept. of Labor* Online, *accessed May 28, 2016*).

[73] This monetary estimate is a lower bound for the cost of time as there is an extensive literature concluding that people place a higher value on time spent waiting than they do on the same amount of time in other circumstances (Maister, 1985; Larson, 1987; Small and Verhoef, 2007; Abrantes and Wardman, 2011). In quantifying the effect of public transit on traffic congestion, Anderson (2014) uses a delay multiplier of 1.8. Using this multiplier would bring the time cost estimates up to \$617 million.

annually. Thus the time savings of issuing "free" bags at checkout in California is in line with the time savings from other policies that affect congestion.

To compare the time cost of DCB policies to the benefits of reducing plastic bag consumption, I use an estimate of how much taxpayers spend in collection, processing, and landfilling disposable bag waste, which is 1.1 cents per bag (Herrera Environmental Consultants, Inc., 2008).[74] Based on the in-store observational data of bag use at checkout, a statewide DCB policy would lead to the use of 4.7 billion fewer disposable bags per year. This would save \$51.7 million in tax dollars annually.[75] However, this tax estimate does not include the environmental cost of plastic marine debris.[76] Thus while the aggregate time cost I estimate surpasses the taxes currently paid by Californians in cleaning up plastic bags, it might not surpass the long run environmental costs of plastic in oceans and waterways.

The Incidence of DCB Policies on Supermarkets

What Share of the DCB Policy Slowdowns Do Supermarkets Bear?

Stores could be hurt if productivity slowdowns lead to lost transactions and lower revenues. Given that the average transaction in my sample costs roughly \$40 to the consumer, if all 18 customers not served per shift because of the DCB policies decide to purchase food elsewhere, stores would lose \$720 in revenue each shift. This would be an extreme case. As suggested by Figure 2.14a, it is likely that some of these transactions spill into the next shift or are made up in other hours of the day/week. I do not have transactions for an entire day, and consequently, cannot measure whether total daily store revenue decreased due to the DCB policies. However, knowing that long waiting time is one of the factors that brings the most dissatisfaction to customers (Nomi, 2014; Katz et al., 1991; Tom and Lucey, 1995; Hirogaki, 2014), grocery stores could choose to open up more registers. To get back to the same level of transactions per shift from before the policies, store would need to open 0.302 more registers.[77] As shown in Figure 2.10, I do not find an increase in the number of registers during the peak 1:00-4:00pm hours. While stores cannot open more registers than they have available, stores can open more registers during the less busy hours shouldering the peak hours. If all 10,935 supermarkets and grocery stores in California opened 0.302 more

[74] The California Senate Rules Committee (2014) cite a lower estimate, with Californian taxpayers spending \$25 million per year to dispose of 14 billion plastic bags, which is \$0.002 per bag. Conversely, a study of the budgets of six major cities in the U.S. cites a higher estimate, with litter control costs of \$0.032 and \$0.079 per bag (Burnett, 2013).

[75] In the in-store data, I find that the average customer uses 3.33 fewer disposable bags per transaction post-DCB policy. Given Californian adults make 1.42 billion grocery transactions per year, this equal 4.7 billion fewer disposable bags per year.

[76] Jambeck et al. (2015) estimate that 1.7-4.6% of the plastic waste generated across 192 coastal countries around the globe is mismanaged and enters the ocean. Once in waterway, plastic bags do not biodegrade, but instead break into smaller pieces, which can be consumed by fish, turtles, and whales that mistake them for food.

[77] The number of additional registers needed to get back to the same level of transactions is calculated using estimates from Table 2.4 as follows: $0.302 = (\frac{556.829}{556.829-18.226} - 1) * 8.931$.

registers during the 14 shoulder hours per week, this would cost $31 million in additional wages per year (at a $12 per hour wage rate).[78] In Figure 2.14b, I find weak evidence that stores do open more registers during the shoulder hours. In summary, while stores have the option of bearing the burden of DCB policies during non-peak hours, customers bear the burden of the policies during peak hours when checkout capacity is constrained.

Supermarket Savings from No Longer Providing "Free" DCBs to Customers

It is important to consider the benefits of DCB policies to retail stores. In particular, DCB policies reduce the need to purchase disposable carryout bags to provide to customers. Standard single-use plastic bags cost retailers on average 3 cents each and paper bags cost 7 to 10 cents each.[79] In interviews, store managers list disposable bags as their fourth largest operating cost, after electricity, payroll, and credit card fees. Retail stores usually pass the cost of disposable bags on to their customers by incorporating them into the overall price of groceries. If stores do not adjust their prices down to account for the bags they no longer buy, stores could experience significant savings. Furthermore, the California DCB policies stipulate that grocery stores must sell paper bags for 10 cents, even if a store purchases bags at a lower price. This 10-cent fee is kept entirely by the store and is not a tax collected by the government. To get a sense of the magnitude of revenue stores would make on paper bags sales under a statewide policy, I use my in-store observational data on bag use at checkout. Given the average customer in the post-policy period buys 0.46 paper bags and that Californian adults make 1.42 billion grocery trips per year, supermarkets would make $65.3 million in revenue from paper bags sales annually.

Reoptimizing Checkout Lanes for Reusable Bags

Finally, supermarkets are optimized for single-use bags, with checkout registers and bagging areas designed for quickly dispensing these bags. It will take time to reoptimize checkout machinery for reusable bags—especially since retailers may want to see how many cities, counties, and states will pass DCB policies before investing in costly store remodels. One potential solution to reduce congestion from DCB policies would be the creation of "Reusable Bag Only" lanes (similar to express checkout lanes and HOV traffic lanes). Since the slow-down in transaction time is driven by those who choose paper bags, separating the paper bag users from the reusable bag users reduces the time externality paper bag users impose on others in line. As a thought experiment, I solve for the number of paper-bag-lanes and non-paper-bag-lanes to make the average wait time per lane proportional to the average

[78] This back-of-the-envelope calculation does not include the potential costs of having to retrain cashiers and baggers to pack varying types of bags.

[79] Bag cost estimates come from interviews with the store managers in my sample, but media articles also confirm these estimates ("Plastic Ban Means Higher Costs are in the Bag." *Crain's Chicago Business.* May 3, 2014. Online, *accessed Sep. 10, 2016*). The cost of paper bags depends on bag size and the presence of handles.

transaction duration for the bag choice in that lane, using summary statistics from the post-policy scanner data.[80] This exercise reveals that a store could convert 22% of its full-service lanes to paper-bag-lanes and 78% to non-paper-bag-lanes and still process the same aggregate number of transactions per shift as if it had not sorted its customers.[81] However, while sorting customers may reduce the time externality of paper bags in theory, in practice sorting customers by bag choice might have consequences for the share of customers purchasing paper bags (e.g., separate bag lanes may increase stigma from purchasing paper bags) and for the duration of paper bag transactions (e.g., separate lanes may increase productivity though specialization), which would alter the results of this thought experiment.

2.8 Conclusion

This study is the first to quantify a hidden time cost of a popular environmental policy aimed at altering consumer behavior. Using detailed scanner data and an event study design, I find that DCB policies lead to a 3% increase in supermarket checkout duration. While I observe evidence of learning at the cashier level, this learning does not offset the slowdown from DCB policies, which persists over the entire sample period. In addition, I find the policy effects are heterogeneous by transaction size and by whether paper bags are purchased, with the largest transactions paying the paper bag fee experiencing a 10% increase in transaction duration.

The slowdown from DCB policies generates a congestion externality, where shoppers not only experience the slowdown of their own transaction, they also experience the slowdown of all transactions ahead of them in line. Using a simple queuing theory model, I calculate that DCB policies cause each checkout line to be roughly 1 customer longer on average during peak hours. This translates to an additional 1.09 minutes of wait and processing time per transaction on average. Aggregating to the state level, a statewide policy would cost Californian shoppers 25.8 million hours annually (≈$343 million).

DCB policies are not the only recent change to cause longer lines at grocery stores. This research has implications for the roll-out of credit cards with chip technology to reduce credit card fraud.[82] An industry study found that using a chip card added 8 to 12 seconds per

[80] The optimal ratio of paper-bag-lanes, L_p, and non-paper-bag-lanes, L_r, is $\frac{L_p}{L_r} = \frac{Share_p * TxnDur_p}{Share_r * TxnDur_r}$, where $Share_p$ and $Share_p$ are the shares of transactions choosing paper and non-paper and $TxnDur_p$ and $TxnDur_r$ are the average transaction durations by bag choice.

[81] The average wait time in non-paper-bag-lanes would be 2.5% shorter than in an unsorted lane because these transactions are 2.5% shorter than the unsorted average. The average wait time for paper-bag-lanes would be 10.2% longer than in an unsorted lane since these transactions are 10.2% longer than the unsorted average.

[82] On Oct. 1, 2015, retailers that did not implement chip payment terminals would face liability for fraudulent charges in their stores for which banks and payment processors were previously liable. As of December 31, 2015, only 20% of retailers had activated terminals and an additional 30% had terminals installed but not activated. Conversely, almost 60% of credit cards issued by banks were embedded with a chip. ("Chip Cards Cause Headaches at Stores Across America." *Bloomberg*. Apr. 13, 2016. Online, *accessed*

checkout transaction.[83] Similar to DCB policies, these slowdowns come from the chip readers being slower than swipe readers and from customers and cashiers needing to learn how the new technology works. The results of this paper imply that the benefits of increased security from chip technology should be compared against the time costs to consumers and retailers.

The policy implications of this paper are threefold. First, policies which incentivize customers to change their habits may have large non-monetary costs, and ignoring these costs overstates the welfare gains of such policies. Even though DCBs comprise a very small fraction of the monetary expenditures for food production, their role in reducing the time and effort necessary for healthy food production is not trivial. Second, not fully considering the institutional conditions and constraints of a policy setting can result in competing externalities. I show that when consumer behavior is connected through queuing systems, individually slower actions propagate into an even larger congestion externality. Third, the policy tool (i.e., bans vs. fees) matters with respect to the time costs. I find that policies which tax both plastic and paper bags have less persistent time costs than policies which ban plastic and tax paper, due to paper bags being the slower technology.

While this paper quantifies a non-monetary cost of an environmental policy, I do not complete a full welfare analysis, nor do I entirely explore all the ways consumers react to this policy. In future work I will examine whether and how consumers circumvent DCB policies in unintended ways—in particular, (i) by switching away from grocery shopping towards eating out more frequently and (ii) by increasing purchases of other types of disposable plastic bags.

Jul. 22, 2016.)

[83] "Visa, Wal-Mart Move to Speed Checkout for Customers with Chip-enable Cards." *Wall Street Journal.* Apr. 19, 2016. Online, *accessed Jul. 22, 2016.*

Chapter 3

Bag "Leakage": The Effect of Disposable Carryout Bag Regulations on Unregulated Bags

3.1 Introduction

Governments often regulate the consumption of products with negative externalities (e.g., alcohol, tobacco, sugar, and gasoline). Leakage occurs when partial regulation results in increased consumption of products in unregulated parts of the economy. If unregulated consumption is easily substituted for regulated consumption, basing the success of a regulation solely on reduced consumption in the regulated market overstates the regulation's welfare gains.

In this article, I quantify leakage from an increasingly popular environmental policy—the regulation of disposable carryout bags (DCB). Many DCB policies prohibit retail food stores from providing customers with thin plastic carryout bags at checkout and require stores to charge a minimum fee for paper carryout bags. However, all remaining types of disposable bags are left unregulated (e.g., garbage bags, food storage bags, and lunch sacks). This article asks the empirical question: Do bans and fees on carryout bags cause consumers to increase their purchases of other unregulated bags?

To answer this question, I bring together two data sources: (i) weekly retail scanner data with store-by-product level price and quantity information, and (ii) transaction level data collected in-store at checkout. Using quasi-random variation in local government DCB policy adoption in California from 2008-2015, I employ an event study design to quantify the effect of DCB policies on the consumption of plastic and paper carryout bags, as well as the consumption of nine other types of disposable bags.

My main results show that a 40 million pound reduction of plastic per year from the elimination of plastic carryout bags is offset by an additional 16 million pounds of plastic from increased purchases of trash bags. In particular, sales of small, medium, and tall trash bags increase by 67%, 50%, and 5%, respectively. This plastic bag "leakage" is an unintended consequence of DCB policies that offsets the benefits of reduced plastic carryout bag use. Additionally, I estimate that DCB policies lead to an additional 69 million pounds of paper annually from increased paper carryout bag use, which is driven by the fact that paper carryout bags are substantially heavier than plastic carryout bags.

These policy-induced changes in plastic and paper bag use have implications for greenhouse gas emissions, marine debris, and landfilling. I conclude this article by comparing the benefits of reduced litter and marine debris from thin plastic carryout bags to the costs of greater emissions from the production of thicker bags, and to the costs of thicker bags taking up more space in landfills. While the upstream relationship between plastic production and carbon footprint is well established, the downstream relationship between plastic litter and marine ecosystems is harder to quantify, making it challenging to evaluate the environmental success of DCB policies. However, it is clear from the results of this article that not examining the leakage effects overstates the regulation's welfare gains.

This article extends the literature on pollution leakage and spillover effects. While numerous studies analyze leakage related to regulating production-driven externalities (such as greenhouse gas emissions),[1] the empirical literature examining leakage from regulating consumption-driven externalities is limited. Adda and Cornaglia (2010) analyze the effect of smoking bans in public places on exposure to second-hand smoke. The authors find that bans displace smokers to private places where they contaminate non-smokers, especially young children. Aguilar et al. (2016) study a countrywide sugary drink tax in Mexico and document a decrease in the consumption of sugar due to the tax. However, they also find an increase in the consumption of fat, sodium, and cholesterol, and no change in overall calories consumed, indicating substitution towards non-taxed goods. Similar to these studies, I find that DCB policies are circumvented by consumers substituting towards unregulated disposable bags.

This article also provides a key variable for the field of life-cycle assessments—studies that estimate a product's cradle-to-grave environmental impact. Life-cycle assessments of plastic and paper carryout bags have been shown to be sensitive to assumptions made about the weight and number of trash bags displaced by the secondary use of plastic carryout bags (Mattila et al., 2011). My results provide an estimate for the reuse of plastic carryout bags—suggesting that at least 12.3% of plastic carryout bags were used as trash bags before the DCB policies went into effect. This estimate can be used as a benchmark for calculating and interpreting life-cycle assessment results going forward.

The remainder of the article is organized as follows. Section 3.2 catalogs the data. Section 3.3 describes the event study empirical design. Section 3.4 presents the main results. Section 3.5 discusses the environmental implications of changes in the composition of plastic and paper bags, with respect to carbon footprint, landfilling, and marine pollution. Section 3.6 concludes.

[1]See Fowlie et al. (2016a,b) for a review of this literature.

3.2 Data

Retail Scanner Data

I use the Retail Scanner Database collected by AC Nielsen and made available through the Kilts Center at The University of Chicago Booth School of Business. The retail scanner data consist of weekly price and quantity information generated by point-of-sale systems for participating retail chains across the United States.[2] I use a subset of retail scanner data from participating stores in California between January 2008 and December 2015. While this database contains a wide variety of store formats and types, I focus my analysis on food stores (i.e., supermarkets, grocery stores, and specialty food stores) and mass merchandising stores (e.g., Walmart and Target) because these stores formats regularly sell non-food grocery items, such as food wrapping materials and bags.

I design a sample of participating stores ideal for the event study model which I present in section 3.3. I include stores in jurisdictions (i.e., counties or cities) that meet all of the following criteria: (1) the jurisdiction is located in California, (2) the jurisdiction implemented a DCB policy between January 2008 and December 2015, and (3) the jurisdiction is either an entire county or can be uniquely identified based on its 3-digit zip code. The third criteria is due to a limitation of the Nielsen scanner data—the exact location of each store is not provided—making it difficult to match stores to DCB policies. I only know in which county and 3-digit zip code each store is located. Thus I limit the sample to the stores in the 5 counties and 2 cities uniquely identified by their 3-digit zip code that implemented DCB policies during my sample period. This gives me a total of 201 stores. Table 3.1 presents characteristics of the seven jurisdictions in my sample, organized by order of DCB policy implementation. In addition to the jurisdiction name, implementation date, and store-sample count, Table 3.1 also reports the 2015 estimated population and median household income for each jurisdiction.

I aggregate the raw microdata to the store-by-month-by-product-group level. With respect to bags, there are 9 product groups: (1) Small Trash Bags, (2) Medium Trash Bags, (3) Tall Kitchen Bags, (4) Large Trash Bags, (5) Sandwich Bags, (6) Freezer Bags, (7) Food Storage Bags, (8) Oven Bags, and (9) Paper Sacks. Table 3.2 presents the summary statistics for the quantity and price variables by product group from 2008 to 2011, which is in the pre-policy for all jurisdictions and stores in my sample. While I am interested in the total number of bags sold, bags are generally sold grouped in boxes. Thus I report summary statistics for both boxes and individual bags. Bag product groups vary greatly in their quantities sold and in their prices. On average, stores in my sample sell 58,892 sandwich bags, 2,319 small trash bags, and 345 oven bags per month. The average box of 26 large trash bags costs $6.58 and the average box of 106 sandwich bags costs $2.72.

[2]When a retail chain agrees to share their data, all of their stores enter the database. As a result, the database includes more than 50,000 individual stores.

Table 3.1: Jurisdiction and Store-Sample Characteristics

Jurisdiction	Implementation Month	# Stores in Sample	Pop. (2015)	Med. HH Inc. (2015)
City of San Jose	January 2012	40	1,026,908	$84,647
San Luis Obispo County	October 2012	17	281,401	$60,691
Alameda County	January 2013	68	1,638,215	$75,619
Mendocino County	January 2013	5	87,649	$42,980
San Mateo County	April 2013	41	765,135	$93,623
City of Richmond	January 2014	5	109,708	$55,102
Sonoma County	September 2014	25	502,146	$64,240

Note: Jurisdictions were chosen based on meeting all of the following criteria: (1) Jurisdiction is located in California, (2) Jurisdiction implemented a DCB policy by December 31, 2015, and (3) Jurisdiction is either an entire county or can be uniquely identified based on its 3-digit zip code. *Sources*: Author's calculation. Population and median household income statistics come from U.S. Census Bureau, Population Estimates Program (PEP) and American Community Survey (ACS). Online, *accessed Apr. 25, 2017*.

In-store Data

The second data source I use is observational data measuring the number and types of carryout bags used at checkout. These transaction level data, collected in-store at checkout, are described in detail in Chapter 1, section 1.3. Table 3.3 provides additional summary statistics for these data with respect to the number of carryout bags used per transaction—by bag type (i.e., plastic, paper, and reusable) and by whether or not the transaction occurred at a store with a DCB policy. The average transaction at a store without a DCB policy used 3.73 plastic bags, 0.04 paper bags, and 0.15 reusable bags, while the average transaction at a store with a DCB policy used 0.00 plastic bags, 0.50 paper bags, and 1.01 reusable bags.

Bag Product Group by Weight

In order to compare the environmental impacts of the various types of bags people use, I convert all bag product groups into their weight in pounds. Table 3.4 describes the material, weight, and volume capacity for the nine categories of bags from the scanner data and for six categories of common carryout bags. Unless otherwise indicated, I calculate bag weights using material densities and standard bag dimensions. Among the trash and storage bags, sandwich bags are the lightest and carry the least volume (0.0038 lb; 0.2 gal) and large trash bags are the heaviest and carry the greatest volume (0.0555 lb; 30 gal). Among the carryout bags, plastic carryout bags are the lightest and carry the least volume (0.0077 lb; 4 gal) while the various reusable bags are heavier and carry greater volumes (0.0606–0.5051 lb; 5–9 gal).

It is important to note that small trash bags are most similar to plastic carryout bags (i.e., the bags banned under Californian DCB policies) with respect to material, weight, and volume capacity.

Table 3.2: Scanner Date Store-by-Month Summary Statistics; Pre-policy (2008-2011)

Variable	Mean	Std. Dev	Min	Max	Obs.
Small Trash Bags (4 gal.)					
Boxes sold per month	52.25	53.51	0.00	439.00	9,136
Bags sold per month	2,318.99	3,514.17	0.00	34,740.00	9,136
Bags per box	35.79	13.59	22.00	112.50	9,012
Price per box	2.89	0.67	1.36	5.49	9,012
Price per bag	0.09	0.02	0.03	0.19	9,012
Medium Trash Bags (8 gal.)					
Boxes sold per month	33.73	31.08	0.00	338.00	9,136
Bags sold per month	1,218.39	2,033.91	0.00	23,850.00	9,136
Bags per box	27.80	16.09	20.00	100.00	8,733
Price per box	3.13	0.86	1.69	5.75	8,733
Price per bag	0.13	0.03	0.03	0.22	8,733
Tall Kitchen Bags (13 gal.)					
Boxes sold per month	412.98	347.56	23.00	3,304.00	9,136
Bags sold per month	21,209.03	21,734.06	1,419.00	190,217.00	9,136
Bags per box	49.77	6.98	31.64	77.57	9,136
Price per box	6.80	0.85	3.88	9.35	9,136
Price per bag	0.16	0.02	0.09	0.25	9,136
Large Trash Bags (30 gal.)					
Boxes sold per month	137.47	85.93	4.00	869.00	9,136
Bags sold per month	3,484.93	2,631.15	140.00	27,556.00	9,136
Bags per box	26.25	4.20	11.67	40.85	9,136
Price per box	6.58	0.90	3.92	9.48	9,136
Price per bag	0.30	0.03	0.16	0.53	9,136
Sandwich Bags					
Boxes sold per month	546.84	512.17	6.00	6,252.00	9,136
Bags sold per month	58,891.85	60,442.03	720.00	743,510.00	9,136
Bags per box	106.01	11.85	66.00	155.63	9,136
Price per box	2.72	0.26	1.59	3.63	9,136
Price per bag	0.04	0.07	0.02	1.22	9,136
Freezer Bags					
Boxes sold per month	324.32	222.32	20.00	2,219.00	9,136
Bags sold per month	8,450.76	7,050.96	461.00	78,028.00	9,136
Bags per box	23.34	3.82	14.67	42.00	9,136
Price per box	3.24	0.54	1.59	6.11	9,136
Price per bag	0.16	0.03	0.08	0.30	9,136

Variable	Mean	Std. Dev	Min	Max	Obs.
Food Storage Bags					
Boxes sold per month	570.28	416.84	40.00	3,928.00	9,136
Bags sold per month	18,999.07	15,815.79	865.00	184,290.00	9,136
Bags per box	30.09	2.34	16.50	45.27	9,136
Price per box	3.23	0.42	1.33	5.61	9,136
Price per bag	0.23	0.20	0.06	2.13	9,136
Oven Bags					
Boxes sold per month	86.06	131.20	0.00	2,968.00	9,136
Bags sold per month	344.58	423.19	0.00	9,385.00	9,136
Bags per box	5.56	1.13	1.00	10.00	9,069
Price per box	3.24	0.59	0.39	5.49	9,069
Price per bag	0.95	0.37	0.14	3.89	9,069
Paper Sacks					
Boxes sold per month	56.27	54.86	0.00	604.00	9,136
Bags sold per month	4,837.58	4,003.18	0.00	35,650.00	9,136
Bags per box	89.01	17.58	31.67	100.00	9,118
Price per box	2.12	0.32	1.21	3.63	9,118
Price per bag	0.03	0.01	0.01	0.16	9,118

Source: Author's calculations from retail scanner data.

Table 3.3: In-Store Data Summary Statistics

Variable	**Mean**	**Std. Dev**	**Min**	**Max**	**Obs.**
Without DCB Policies					
Plastic bags per txn.	3.73	3.71	0.00	30.00	2,017
Paper bags per txn.	0.04	0.39	0.00	8.00	2,017
Reusable bags per txn.	0.15	0.63	0.00	7.00	2,017
With DCB Policies					
Plastic bags per txn.	0.00	0.00	0.00	0.00	2,407
Paper bags per txn.	0.50	1.19	0.00	14.00	2,407
Reusable bags per txn.	1.01	1.42	0.00	10.00	2,407

Source: Author's calculations from in-store observational data.

Table 3.4: Bag Product Group Characteristics

Bag Product Group	**Material**	**Weight (lb/bag)**	**Volume Capacity (gal/bag)**
Trash & Storage Bags			
Small trash bag	LDPE; 18in×17in× 0.5mil	0.0101	4
Medium trash bag	LDPE; $20\frac{1}{2}$in×20in×0.69mil	0.0187	8
Tall kitchen bag	LDPE; 24in×$28\frac{3}{8}$in×0.78mil	0.0351	13
Large trash bag	LDPE; 30in×33in×0.85mil	0.0555	30
Sandwich bag	LDPE; $6\frac{1}{2}$in×$5\frac{7}{8}$in×1.50mil	0.0038	0.2
Freezer bag	LDPE; $10\frac{1}{2}$in×$10\frac{3}{4}$in×3.00mil	0.0224	1
Food storage bag	LDPE; 13in×15in×1.75mil	0.0225	2
Oven bag	Nylon; 22in×20in×1.18mil	0.0428	8
Paper sack[1]	Kraft Paper	0.0220	1
Carryout Bags			
Plastic carryout bag[2]	HDPE	0.0077	4
Paper carryout bag[3]	Kraft Paper; Flat Handles	0.1267	5
Reusable carryout bag	Woven PP[4]	0.3086	6
–	Non-woven PP[5]	0.2372–0.2736	5–6
–	Cotton[5]	0.1735–0.5051	5–9
–	Heavy duty LDPE[5]	0.0606–0.0937	5–6

Note: LDPE = low-density polyethylene. HDPE = high-density polyethylene. PP = polypropylene. LDPE has a density of 0.0330 lb/in^3 (Sterling Plastics, Inc. Online, *accessed Apr. 25, 2017*). HDPE has a density of 0.0347 lb/in^3 (Plastics International. Online, *accessed Apr. 25, 2017*). Nylon has a density of 0.0412 lb/in^3 (AZO Materials. Online, *accessed Apr. 25, 2017*). mil = a thousandth of an inch. Unless otherwise indicated, bag weights are calculated by author using material densities and standard bag dimensions.

[1] *Source*: Uline. "Paper Grocery Bags – 6 × $3\frac{5}{8}$ × 11", #6, Kraft" Online, *accessed Apr. 25, 2017.*

[2] *Source*: CalRecycle. "Diversion Study Guide, Appendix I; Conversion Factors: Glass, Plastic, Paper, and Cardboard." Online, *accessed Apr. 25, 2017.*

[3] *Source*: Uline. "Paper Grocery Bags – 12 × 7 × 14", 17 Barrel, Flat Handle, Kraft" Online, *accessed Apr. 25, 2017.*

[4] *Source*: ReuseThisBag.com. "Woven Polypropylene Grocery Bag." Online, *accessed Apr. 25, 2017.*

[5] *Source*: Environment Agency. "Life Cycle Assessment of Supermarket Carrier Bags: A Review of the Bags Available in 2006." Online, *accessed Apr. 25, 2017.*

3.3 Empirical Design

Scanner Data Event Studies

I estimate the causal effect of DCB policies on bag purchases using an event study design. I aggregate the raw retail scanner data to the store-by-month-by-product-group level and employ the following event study regression model:

$$Y^B_{sjm} = \sum_{l=-8}^{8} \beta_l D_{l,jm} + \theta_{sj} + \delta_m + \epsilon_{sjm} \tag{3.1}$$

where Y^B_{sjm} is the outcome variable for store s in jurisdiction j and month-of-sample m with respect to bag product group B, θ_{sj} is a vector of store fixed effects, and δ_m is a vector of month-of-sample fixed effects. $D_{l,jm}$ is a dummy variable equaling one if jurisdiction j in month m implemented a DCB policy l months ago, with $l = 0$ denoting the month of implementation. The endpoints are binned, with $D_{8,jm} = 1$ for all months in which it is 8 months or more since DCB policy implementation and, similarly, $D_{-8,jw} = 1$ for all months in which it is 8 months or more until implementation. The week prior to implementation ($l = -1$) is the omitted category. Store fixed effects control for time-invariant store level characteristics (i.e., store size, number of registers, types of departments offered). Month-of-sample fixed effects control for variation over time that effect all stores (i.e., holidays and seasons). The primary outcome variables I use for Y^B_{sjw} will be the number of product group B bags sold in store s and month-of-sample m.

The β_l vector is the parameter of interest, as it traces out the differences in outcomes from before the DCB policies to after. I hypothesize that sales of bags deemed by customers to be substitutes for plastic carryout bags will increase. Thus, for any product group B that is a substitute for plastic carryout bags, I would expect the β_l coefficients in the post-policy period to be greater than zero.

The identifying assumption of the model is that, absent the DCB policies, outcomes at the treated stores would have remained similar to the stores yet to be treated. Underlying trends in the outcome variable correlated with DCB policy enactment are the most likely violation of this assumption. Part of the appeal of event study designs is that the pre-policy portion of the β_l vector provides a check against this possible violation. If DCB policies are unassociated with underlying trends, there should be no trend in the β_l vector in the pre-policy period.

In-store Data Event Studies

To examine the effects of DCB policies on the use of various carryout bags, I use the in-store, transaction level data to estimate the following event study model:

$$Y^C_{tsjdm} = \sum_{l=-1}^{3} \beta_l D_{l,jm} + \beta_x X_{tsjdm} + \theta_{sj} + \delta_{dm} + \epsilon_{tsjdm} \tag{3.2}$$

where Y^C_{tsjdm} is the outcome variable for transaction t in store s on date d in month m with respect to carryout bag type C, $D_{l,jm}$ is the set of monthly event study dummies, X_{tsjdm} are control variables, θ_{sj} are store fixed effects, and δ_{dm} are date fixed effects. The control variables include indicators for the gender and race of the person paying, whether there was a checkout interruption, and whether a bagger was present. The primary outcome variables I use for Y^C_{tsjdm} will be the number of carryout type C bags used per transaction.

3.4 Results

Scanner Data Results

The figures in this section present the results from the estimation of event study Equation 3.1, where the $\hat{\beta}_l$ point estimates and 95% confidence intervals are displayed graphically.[3] Unless specified otherwise, I cluster the standard errors at the store level to account for the possibility that the errors are correlated within a given store, but not across stores.[4]

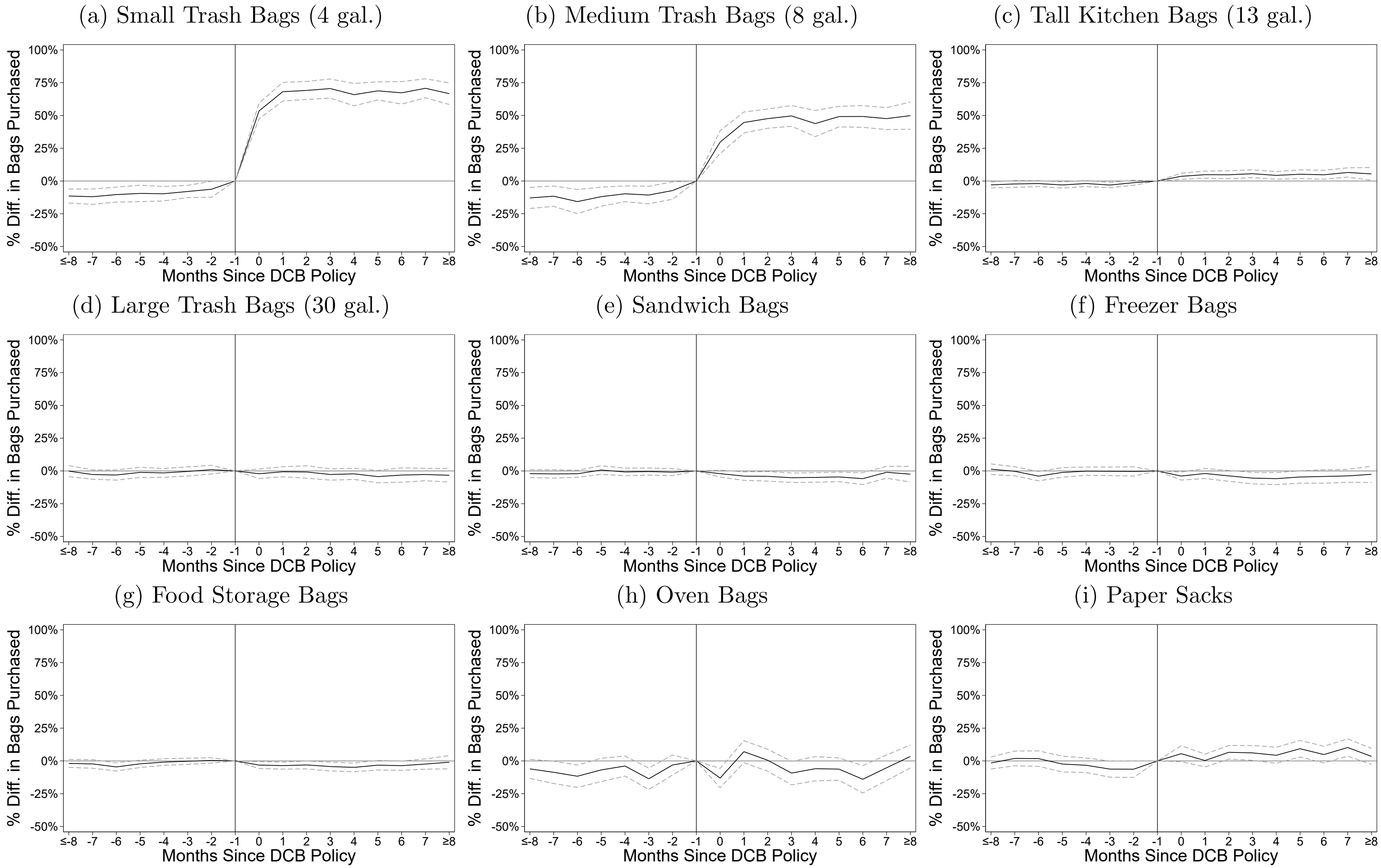
In Figure 3.1, the scanner data are averaged to the store-by-month level for each product, for a total of 17,906 observations. The outcome variable, Y^B_{sjm}, is the logged number of product group B bags sold in store s and month-of-sample m, which means the $\hat{\beta}_l$ point estimates measure the percent difference in bag sales between treated and yet-to-be-treated stores l weeks from DCB policy implementation. The panels of Figure 3.1 correspond to the following bag products: (a) small trash bags, (b) medium trash bags, (c) tall kitchen bags, (d) large trash bags, (e) sandwich bags, (f) freezer bags, (g) food storage bags, (h) oven bags, and (i) paper sacks.

Among the nine event studies presented in Figure 3.1, panels (a) and (b) stand out. In panel (a), I find that the DCB policies lead to a large and significant increase in sales of small trash bags. The jump in sales begins immediately after policy implementation, with $\hat{\beta}_0 = 0.534$ and $\hat{\beta}_1 = 0.681$. These estimates mean that the average monthly sales of small garbage bags at treated stores are 53.4% and 68.1% higher during the first and second months of a DCB policy. The increase in sales remains constant over time, ending with $\hat{\beta}_8 = 0.666$. The $\hat{\beta}_8$ coefficient indicates that for all months in which it has been 8 or more months since DCB policy implementation, sales of small garbage bags at treated stores remain 66.6% higher than at the yet-to-be-treated stores. All of the post-policy $\hat{\beta}_l$ coefficients are significantly greater than zero at the 1% significance level. Importantly, the the pre-policy $\hat{\beta}_l$ coefficients are close to zero and nearly parallel to the x-axis, which provides evidence in favor of the identifying assumption that small garbage bag sales were not trending before the DCB policies went into effect.

[3] I estimate all fixed-effect equations in STATA using the command `reghdfe` (Correia, 2014).

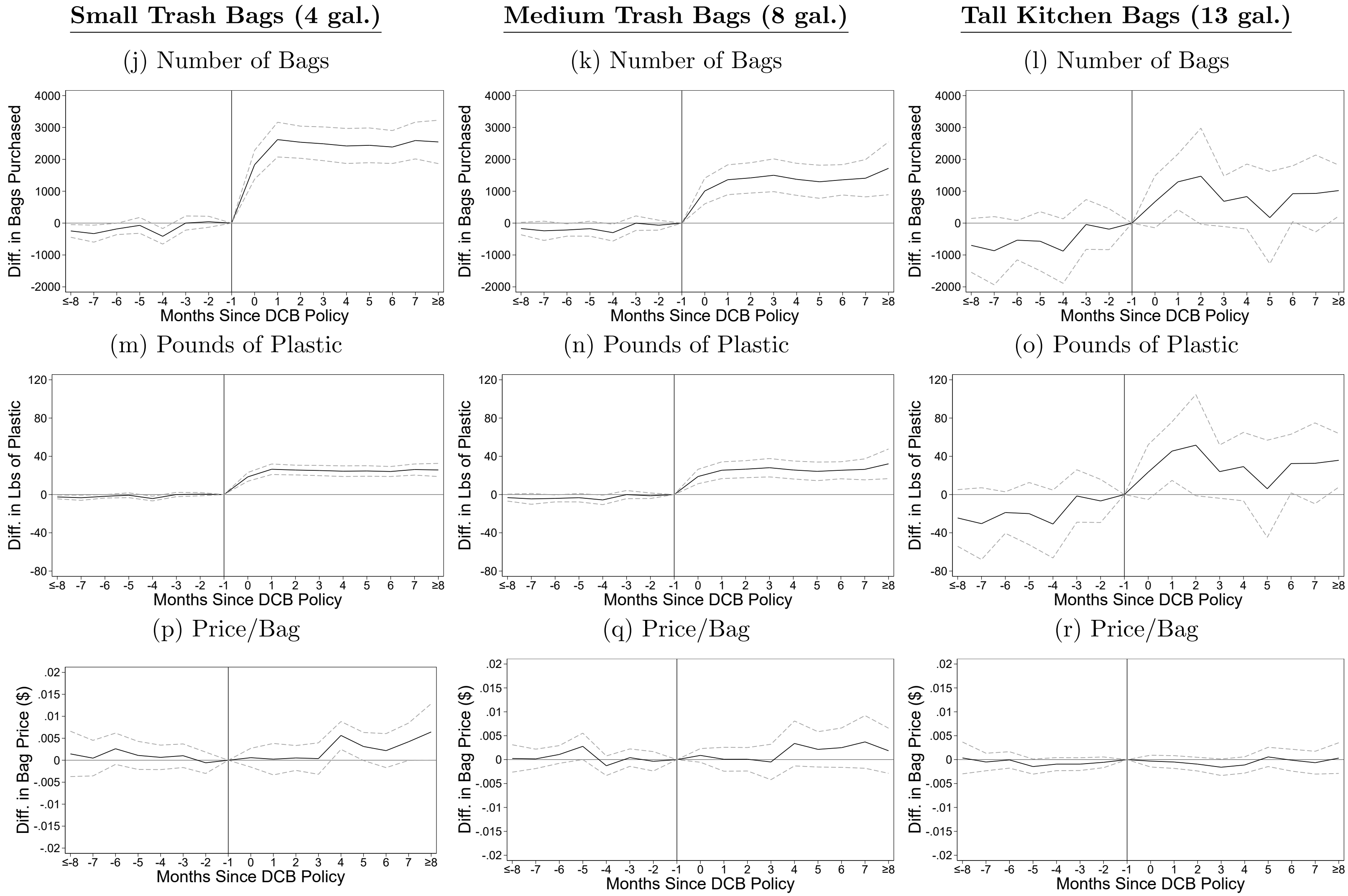
[4] Ideally, standard errors would be clustered at the jurisdiction level, since that is the level of treatment. However, with only 7 jurisdictions this would lead to biased standard errors. Clustering the standard errors at the jurisdiction-by-month-of-sample level instead of at the store level does not change the significance of the results.

Figure 3.1: Effect of DCB Policies on Bag Purchases (*Scanner Data*)



Note: The figure panels display the $\hat{\beta}_l$ coefficient estimates from event study Equation 3.1. The dependent variable is logged number of product group B bags sold in store s, jurisdiction j, and month-of-sample m. Upper and lower 95% confidence intervals are depicted in gray, estimated using standard errors clustered at the store level.

Figure 3.2: Effect of DCB Policies on Trash Bag Purchases (*Scanner Data*)



Note: The figure panels display the $\hat{\beta}_l$ coefficient estimates from event study Equation 3.1. The dependent variables for store s in jurisdiction j and month-of-sample m are: panels (j) to (l)—the number of product group B bags sold; panels (m) to (o)—the pounds of product group B bags sold; and panels (p) to (r)—the price of product group B bags sold. Upper and lower 95% confidence intervals are depicted in gray, estimated using standard errors clustered at the store level.

The results in panel (b) for medium trash bags follow a similar pattern as those in panel (a) for small trash bags. I find that average monthly sales of medium trash bags are 29.7% higher during the first month of a policy, 44.6% higher in the second month, and remain 49.9% higher 8 months or more after a policy. In panel (c), I also find a small increase in the sale of tall kitchen bags that corresponds to the implementation of DCB policies. Monthly sales of tall kitchen bags are 3.6% higher in the first month of a policy, 4.8% higher in the second month, and 5.4% higher 8 months or more after a policy.[5]

The remaining six bag product groups in Figure 3.1 do not experience significant or persistent increases in sales that are contemporaneous with policy implementation. All together, these results provide strong evidence that the elimination of plastic carryout bags due to DCB policies lead costumers to substitute towards purchasing more trash bags, and in particular, small and medium trash bags which are close in size and carrying capacity to plastic carryout bags. These results also indicate that some customers are willing to pay for the trash bag services they gained from "free" plastic carryout bags.

To understand the magnitude of the changes in the sales of trash bags, I estimate Equation 3.1 with the outcome variables in levels instead of logs. Figure 3.2 presents the results of the event study model for small, medium, and tall kitchen trash bags. In panels (j), (k), and (l), I find that DCB policies cause a 2,547 bag increase in small garbage bag purchases per store-month,[6] a 1,719 bag increase in medium garbage bag purchased per store-month,[7] and a 1,020 bag increase in tall kitchen bags purchased per store-month.[8]

In panels (m), (n), and (o) of Figure 3.2, I convert the bag types into their weight equivalents. DCB policies lead to 26, 32, and 36 additional pounds of plastic consumed per store-month from increased purchases of small, medium, and tall kitchen trash bags respectively. In section 3.5, I further discuss the environmental implications of these policy-induced changes in the consumption of plastic bags, with respect to carbon footprint, landfilling, and marine pollution.

In order to rule out the alternative hypothesis that changes in bag prices are driving the changes in bag demand, I examine whether the price of bags change with DCB policy implementation in panels (p), (q), and (r) of Figure 3.2. I find no changes in bag price that are contemporaneous with policy implementation. However, four months after the policy implementation, the price of small trash bags at treated stores begins to increase. In panel (p), $\hat{\beta}_8 = 0.006$, which approximately is a 4% increase in the price per small garbage bag. This is consistent with suppliers of trash bags responding to the exogenous change in small trash bag demand by increasing their prices.

[5]The only other estimate of changes in trash bags sales due to DCB policies comes from Ireland, where retailers self-reported a 77% increase in small trash bag sales and no change in larger trash bag sales (Nolan ITU, 2002).

[6]$\hat{\beta}_8 = 2547.237$ in Figure 3.2(j).

[7]$\hat{\beta}_8 = 1718.857$ in Figure 3.2(k).

[8]$\hat{\beta}_8 = 1020.362$ in Figure 3.2(l).

In-store Data Results

The figures in this section present the results from the estimation of event study Equation 3.2, where the $\hat{\beta}_l$ point estimates and 90% confidence intervals are displayed graphically. I cluster the standard errors at the store-day level to account for the possibility that the errors are correlated within a given store and day, but not across stores and days.

In the top three panels of Figure 3.3, outcome variable Y^C_{tsjdm} is the number of bags sold of carryout bag group C in transaction t, store s, jurisdiction j, day d, and month m. This means the $\hat{\beta}_l$ point estimates measure the difference in bag usage between treated and control stores l months from the DCB policy implementation. The panels of Figure 3.3 correspond to the following carryout bag groups: (a) plastic carryout bags, (b) paper carryout bags, and (c) reusable carryout bags.

As expected, I find that the DCB policies lead to a large and significant decrease in use of plastic carryout bags. On average, customers use 3.7 fewer plastic carryout bags when DCB policies go into effect.[9] This reflects the fact that DCB policies prohibit the use of plastic carryout bags and that customers used 3.73 bags per transaction on average before DCB policies were implemented (Table 3.3). DCB policies also lead to significant increases in the usage of paper and reusable carryout bags. When policies are implemented, customers use 0.4 more paper bags and 0.8 more reusable bags per transaction.[10]

In panels (d), (e), and (f), I convert the bag types into their weight equivalents. DCB policies lead to 0.031 fewer pounds of plastic per transaction from the elimination of plastic carryout bags and 0.043 additional pounds of paper per transaction from the increased use of paper carryout bags.[11] Thus, with respect to weight, the elimination of plastic is more than offset by the increased use of paper. I also find that the average transaction is using an additional 0.148 pounds of reusable bags per transaction.[12] How many times paper and reusable bags are reused, and how they are disposed, will have major implications for the success of these policies. In section 3.5, I further discuss the environmental implications of these policy-induced changes in the usage of carryout bags, with respect to carbon footprint, landfilling, and marine pollution.

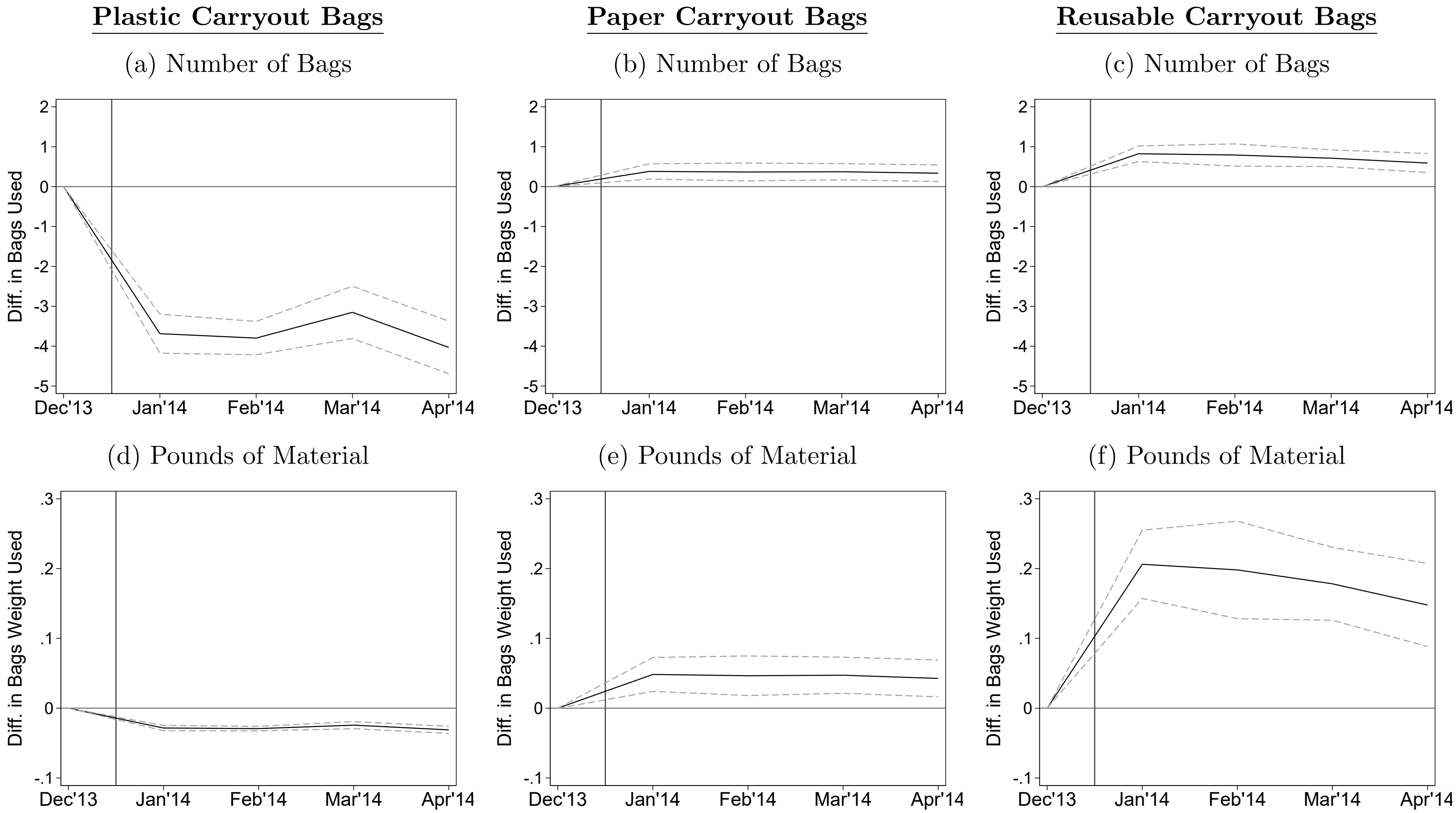
[9] $\hat{\beta}_0 = -3.686$ in Figure 3.3(a)

[10] $\hat{\beta}_0 = 0.382$ in Figure 3.3(b). $\hat{\beta}_0 = 0.825$ in Figure 3.3(c).

[11] $\hat{\beta}_3 = -0.031$ in Figure 3.3(d). $\hat{\beta}_3 = 0.043$ in Figure 3.3(e).

[12] $\hat{\beta}_3 = 0.148$ in Figure 3.3(f).

Figure 3.3: Effect of DCB Policies on Carryout Bag Use (*In-store Data*)



Note: The figure panels display the $\hat{\beta}_l$ coefficient estimates from event study Equation 3.2. The dependent variables for transaction t in store s, jurisdiction j, day d, and month m are: panels (a) to (c)—the number of product group C bags used; and panels (d) to (f)—the pounds of product group C bags used. Upper and lower 90% confidence intervals are depicted in gray, estimated using standard errors clustered at the store-by-date level.

3.5 Discussion

The event study results above show that DCB policies lead to decreased use of plastic carryout bags, increased use of paper and reusable carryout bags, and increased purchases of garbage bags. To evaluate the environmental impacts of these changes in bag use, I convert the changes in bag use into pounds and calculate the annual change in pounds of material used per year in California. Table 3.5 presents these calculations. Columns (1) and (2) present the changes in bag usage from the estimation of event study Equations 3.1 and 3.2, as shown in Figures 3.2 and 3.3. For the trash bag products, the $\hat{\beta}_8$ estimates are used (column 1) and for the carryout bag products, the $\hat{\beta}_3$ estimates are used (column 2). In column (3), I aggregate the estimates in columns (1) and (2) to the annual California level. To make this aggregation for trash bags, I use the estimate that California had 14,286 food stores in 2014—10,891 supermarkets, other grocery stores and specialty food stores and 3,395 general merchandise stores.[13] To make this aggregation for carryout bag, I use the estimate that Californian adults make 1.42 billion grocery transactions per year.[14] Finally, in column (4) I calculate the changes in the pounds of material consumed per year in California using the bag material and weight information from Table 3.4.

Table 3.5 reveals that DCB policies lead to a 40.3 million pound reduction in plastic per year in California from decreased use of plastic carryout bags. However, this reduction is offset by a 16.0 million pound increase in plastic from additional purchases of trash bags—4.4 million, 5.5 million, and 6.1 million pounds from small, medium and tall kitchen trash bags respectively. Thus, DCB policies have a 39.7% leakage rate with respect to pounds of plastic. In other words, 39.7% of the plastic reduction benefits of DCB policies are lost due to consumption shifting towards unregulated bags. Furthermore, DCB polices lead to a 68.7 million pound increase in paper per year in California from increased use of paper carryout bags. Therefore, on net DCB policies lead to 44.4 million additional pounds of bags.

How do the changes in bag usage presented in Table 3.5 compare to average annual bag usage in California? According to CalRecycle, Californians dispose of 766.3 million lbs of plastic trash bags, 314.8 million lbs of plastic grocery and other merchandise bags, and 141.3 million lbs of paper bags each year.[15] Comparing these averages to column (5) of Table 3.5, DCB policies lead to a 2.1% increase in the use of trash bags, a 12.8% decrease in the use of plastic grocery bags and other merchandise bags, and a 48.6% increase in the use of paper bags.

The results in Table 3.5 reveal that DCB policies are shifting consumers towards fewer but heavier bags. While 3,785 million fewer bags are estimated to be used per year in

[13]*Source*: U.S. Census Bureau, 2014 County Business Patterns. Online, *accessed Apr. 25, 2017.*

[14]Hamrick et al. (2011) estimate how much time Americans spend on food and find that the average adult in the U.S. grocery shops once every 7.19 days, which is 50.74 times per year. According to the 2010 Census, there are 28 million adults in California. Thus Californian adults make roughly 1.42 billion trips to the grocery store annually.

[15]*Source*: "2014 Disposal-Facility-Based Characterization of Solid Waste in California." *CalRecycle*. Online, *accessed Apr. 25, 2017.*

Table 3.5: Effect of DCB Policies on Annual Bag Usage and Weight in California

	(1) **Δ Bags/ Store-Month**[1]	(2) **Δ Bags/ Txn**[1]	(3) **Δ Bags/ Year**[2]	(4) **Δ Lbs/ Year**[3]
Trash Bags				
Small trash bag	2,547		437 million	4.4 million
Medium trash bag	1,719		295 million	5.5 million
Tall kitchen bag	1,020		175 million	6.1 million
Carryout Bags				
Plastic carryout bag		-3.686	-5,234 million	-40.3 million
Paper carryout bag		0.382	542 million	68.7 million
Net Δ			-3,785 million	44.4 million

[1] *Note*: Changes in bag usage come from the estimation of event study equations 3.1 and 3.2, as show in figures 3.2 and 3.3. For the trash bag products, I present the $\hat{\beta}_8$ estimates and for the carryout bag products, I present the $\hat{\beta}_3$ estimates.
[2] *Note*: Changes in trash bag usage is calculated using the estimate that California had 10,891 supermarkets, other grocery stores and specialty food stores and 3,395 general merchandise stores in 2014, for a total of 14,286 food stores (*source*: U.S. Census Bureau, 2014 County Business Patterns. Online, *accessed Apr. 25, 2017*). Changes in carryout bag usage is calculated using the estimate that Californian adults make 1.42 billion grocery transactions per year. Hamrick et al. (2011) estimate how much time Americans spend on food and find that the average adult in the U.S. grocery shops once every 7.194 days, which is 50.74 times per year. According to the 2010 Census, there are 28 million adults in California. Thus Californian adults make roughly 1.42 billion trips to the grocery store annually.
[3] *Note*: Changes in the pounds of material per year are calculated using the bag material and weight information from Table 3.4.

California, the weight of bags used is 44.4 million pounds greater. This result is concerning with respect to planet-warming emissions, given the carbon footprint of an object is generally proportional to its mass.[16] A UK Environmental Agency (2011) study calculated the global warming potential (measured in kilograms of CO2 equivalent) of various plastic, paper, and reusable carryout bags. They found that to have the same global warming potential as a traditional plastic carryout bag, a paper carryout bag would need to be used 3 times, a low-density polyethylene (LDPE) reusable bag (the same material as trash bags) would need to be used 4 times, a non-woven polypropylene (PP) reusable bag would need to be used 11 times, and a cotton reusable bag would need to be used 131 times.

My results also provide a lower bound for the reuse of plastic carryout bags—suggesting that at least 12.3% of plastic carryout bags were used as trash bags before the DCB policies went into effect. This is an important estimate in itself because life-cycle assessments have

[16] *Source*: "Banning Plastic Bags is Great for the World, Right? Not So Fast." *Wired*. Jun. 10, 2016. Online, *accessed Apr. 25, 2017*.

been shown to be sensitive to assumptions made about the weight and number of trash bags displaced by the secondary use of plastic carryout bags (Mattila et al., 2011). For instance, the UK Environmental Agency (2011) study estimated that if 40% of plastic carryout bags were reused once as a trash bin liner, a paper carryout bag would need to be used 4 times to have the same global warming potential, a LDPE bag would need to be used 5 times, a non-woven PP bag would need to be used 14 times, and a cotton bag would need to be used 173 times. Thus my results provide an important variable in calculating and interpreting life-cycle assessment results.

Life-cycle assessments of carryout bags, such as UK Environmental Agency (2011) study, have constantly found that plastic carryout bags take significantly less energy and water to produce, require less energy to transport, and emit fewer greenhouse gases in their production than paper and other types of reusable bags (Freinkel, 2011).[17] However, while life-cycle assessments do well measuring energy-related impacts, they have trouble with less easily quantified issues, such as litter and marine debris, the toxicity of materials, and impacts on wildlife (Freinkel, 2011). Jambeck et al. (2015) calculate that 1.7-4.6% of the plastic waste generated in coastal countries around the globe is mismanaged and enters the ocean. Plastic carryout bags are particularly problematic because they are lightweight and aerodynamic, which make it easy for them to blow out of waste streams (even when properly disposed) and into the environment and waterways. The United Nations Environmental Programme (2014) estimates the environmental damage to marine ecosystems of plastic litter is $13 billion per year. This estimate includes financial losses incurred by fisheries and tourism as well as time spent cleaning up beaches. While plastic bags and films represent only 2.2% of the total waste stream (CA Senate Rules Committee, 2014), plastic carryout bags and other plastic bags are the eighth and sixth most common item found in coastal cleanups.[18] Once in waterways, plastic bags do not biodegrade, but instead break into smaller pieces, which can be consumed by fish, turtles, and whales that mistake them for food. A survey of experts, representing 19 fields of study, rank plastic bags and plastic utensils as the fourth severest threat to sea turtles, birds, and marine animals in terms of entanglement, ingestions, and contamination (Wilcox et al., 2016).

Plastic trash bags, on the other hand, are less likely to blow out of waste streams because they are weighed down by the trash they carry. With respect to my results in Table 3.5, this means a statewide DCB policy would lead to 40 million fewer pounds of plastic carryout bags that could end up in storm drains and oceans, and 16 million additional pounds of plastic trash bags that are more likely to remain in landfills. While a handful of studies have

[17]The negative environmental impacts of paper bags include: paper bags are more energy and water intensive to manufacture than plastic bags; paper bag production generates 70% more air and 50 times more water pollutants than the production of plastic bags; it takes 98% less energy to recycle a pound of plastic than a pound of paper; and paper bags are 9 times heavier than plastic bags, requiring more space in transportation trucks and landfills (*Source*: "Graphic: Paper or Plastic?" *The Washington Post*. Oct. 3, 2007. Online, *accessed Apr. 25, 2017*.

[18]*Source*: "International Coastal Cleanup. Annual Report 2016." *Ocean Conservancy*. Online, *accessed Apr. 25, 2017*.

found evidence that DCB policies lead to less liter in waterways,[19] no study has examined whether DCB policies lead to changes in the amount of plastic entering landfills and how this affects the cost of landfilling.

In summary, in evaluating the environmental success of DCB policies, the benefits of reduced litter and marine debris needs to be compared to the costs of greater greenhouse gas emissions and thicker plastics going into landfills. While the upstream relationship between plastic production and carbon footprint is well understood, the downstream relationship between plastic litter and marine ecosystems is less established. Moreover, it is challenging to quantify the emotional costs of litter. "Data-driven comparisons don't speak to our feelings about the two materials—our irrational sense of comfort with the feel of paper bags and our sense of discomfort with plastic's preternatural endurance. The presence of plastic where it doesn't belong—matter out of place—pisses people off" (Freinkel 2011, p. 159). If carbon footprint was the only metric of environmental success, the results in Table 3.5 suggest DCB policies are having an adverse effect. However, if the unmeasured benefits with respect to marine debris, toxicity, and wildlife are great enough, they could outweigh the greenhouse gas costs.

3.6 Conclusion

This article is the first to evaluate how regulating the use plastic and paper carryout bags affects the sale of unregulated disposable bags. Using quasi-random variation in local government policy adoption in California in an event study design, I find that the banning of plastic carryout bags leads to significant increases in the sale of trash bags, and in particular small trash bags. When converted into pounds of plastic, nearly 40% of the plastic reduction from DCB policies is lost due to consumption shifting towards unregulated plastic bags. Moreover, the increase in pounds of paper used from paper carryout bags more than offsets the decrease in pounds of plastic, which has negative implications with respect to the carbon footprint of DCB policies.

Overall, my result suggest that DCB policies are shifting consumers towards fewer but heavier bags. The question remains: Do the benefits of reduced litter and marine debris outweigh the costs of greater greenhouse gas emissions and thicker plastics going into landfills? In order answer this question and evaluate the environmental success of DCB policies, future research is needed on the costs and benefits of plastic marine debris reduction.

[19] The City of San Jose performed creek and river litter surveys before and after the implementation of its 2012 DCB policy. These surveys indicated that plastic carryout bags comprised 8.2% of litter in 2011 and 3.7% of litter in 2012 (City of San Jose Transportation & Environment Committee, 2012). Alameda County found the number of plastic bags observed in its storm drains decreased by 44% after its DCB policy went into effect. (EOA, Inc., 2014)

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Appendix

A.1 Customer Level Sensitivity Analysis

In this appendix, I examine whether the main results are sensitive to the inclusion of customer fixed effects, using scanner data averaged to the customer-month frequency. The event study model at the customer-month level is as follows:

$$Y_{isjm} = \sum_{l=-8}^{6} \beta_l D_{l,jm} + \beta_x X_{isjm} + \gamma_{isj} + \chi_m + \epsilon_{isjm} \quad (3)$$

The outcome variable, Y_{isjm}, is the logged average transaction duration for customer i in store s, jurisdiction j, and month-of-sample m. The control variables include the average number of items scanned, the average amount spent, the average types of items purchased by customer i in store s and month m, as well as month-of-sample and customer fixed effects. Additionally, I control for the number of months the customer has appears in the sample. Including customer fixed effects, γ_{isj}, means the β_l coefficients in Equation 3 measure the policy effects *within customers* over time.

It is important to note that I designed the data to capture stores and cashiers over time, and not necessarily customers over time. I have every transaction during the 1:00-4:00pm shift on Saturdays and Sundays for 53 stores and 3.5 years. If a customer happens to shop at the same time each week or month *using their rewards card*, I can see them multiple times in the sample using the identification code of their card. If they do not use a customer card or they shop at different times each week, I cannot track the customer.

There are 2,146,248 unique customer codes in my sample. 48% of customers appear only once, 24% of customers appear 2-3 times, 12% appear 4-6 times, and the remaining 16% appear 7 times or more. I drop all customers that appear more than 100 times, which is less than half a percentage point of customers. I do this because high card frequencies may be driven by cashiers scanning a store-owned rewards card, or their own personal rewards card, when customers do not have a card. I also drop the customers that appear less than twelve times in the sample, in order to focus on customers that are in the sample long enough to experience learning. As such, the results I present show how DCB policies affect checkout speed within customers, for the 143,000 customers that shop regularly.

Appendix Figure A.1 presents the event study results from estimating Equation 3. The coefficients found in panel (a) at the customer-level are similar to those at the cashier-level in

Figure 2.11. During the first month of the policy, transactions are 6.5% longer ($\hat{\beta}_0 = 0.065$). Transactions that occur 6 months or more after the policy implementation remain 4.0% longer ($\hat{\beta}_6 = 0.040$). Using Wald tests to compare the coefficients, I can reject that all $\hat{\beta}_l$ coefficients in the post-policy period are the same as one another and I can reject that $\hat{\beta}_0 = \hat{\beta}_6$. Therefore, I conclude that the customers who shop regularly do become faster at checking out over time after the policies, however this learning does not offset the initial slowdown in due to the policies.

I next split the customers at treated stores into four groups by whether they *ever* buy paper bags and by whether they buy fewer than 13 items on average.[20] Panels (b) to (e) of Appendix Figure A.1 present the results of Equation 3 estimated separately for each of these treated groups. Importantly, the never-treated (i.e., pure control) customers are the same in each panel. I find no statistically significant slowdown due to the DCB policies for the smaller transaction customers that never purchase paper (panel b). For smaller transactions that ever purchase paper, the DCB policies lead to a temporary slowdown, with a $\hat{\beta}_0 = 0.073$ (panel c). Conversely, I find a persistent 4.1% slowdown in transaction duration for the larger transaction customers that never purchase paper (panel d), and an 6.3% slowdown for the larger transaction customers that ever purchase paper (panel e). These heterogeneity results match what I found when using the store-week data in Figure 2.8. This is reassuring because, at the store-week level, splitting the transactions by whether a paper bag was purchased in the post-policy period meant that the treated customers in the pre-period were not necessarily the same as the treated customers in the post-period. However, the near-zero pre-policy $\hat{\beta}_l$ coefficients in every panel in Appendix Figure A.1 indicates that before the policy, none of the treated customers differed in transaction duration from the control customers. Yet after the policy, treated customers with larger transactions that choose paper bags have significantly longer transaction durations than control customers.

I also estimate Equation 3 with the following outcome variables: (1) average number of items bought per transaction, (2) average amount spent per transaction, (3) average amount spent per item, (4) share of transactions completed at self-checkout registers, and (5) the number of transactions completed per month. In none of these regressions do I find significant changes in the outcome variable that is contemporaneous with the policy change. Thus for the sample of customers that shop regularly, the DCB policies do not appear to be altering the amount of items they buy or the amount they frequent a store during the 1:00-4:00pm weekend shift.[21]

[20] 25% of customers in treated stores purchase smaller transactions and never purchase paper bags, 24% purchase smaller transactions and purchase a paper bag at least once in the sample, 22% purchase larger transactions and never purchase paper bags, and the remaining 29% purchase larger transactions and purchase a paper bag at least once in the sample.

[21] While not statistically different from zero, there is evidence that customers shop fewer times per month during the 1:00-4:00pm weekend shift in the post-policy period. This is consistent with customers choosing to shop at different times of the day or at different stores, or customers remaining in line longer and spilling over into the next shift.

A.2 Correlates of Customer Paper Bag Choice

To understand what factors correlate with choosing to pay the paper bag fee, I estimate the following model using post-policy customer level data at treated stores:

$$Paper_{isjm} = \beta_x X_{isjm} + \sum_{l=0}^{24} \beta_l D_{l,jm} + \gamma_{isj} + \chi_m + \epsilon_{isjm} \tag{4}$$

where $Paper_{isjm}$ is the share of customer i's transactions in store s, jurisdiction j, and month m where at least one paper bag is purchased. X_{isjm} is a set of customer level and store level covariates, $D_{l,jm}$ is a set of months-since-policy dummy variables, γ_{isj} are customer fixed effects, and χ_m are month-of-sample fixed effects. Appendix Table A.3 presents the results, with only month-of-sample fixed effects and months-since-policy fixed effects in column (1) and additionally customer fixed effects in column (2).

The main takeaway from Appendix Table A.3 is that paper bag use in the post-policy period is positively correlated with income, transaction size, and the purchase of more expensive items. Similar to what was shown in Figure 2.6, I find that paper bag use increases with transaction size, except for the largest transactions in Q4, which are less likely to choose paper than Q3. Also, as the amount spent per item increases, paper bag use increases. Both with and without customer fixed effects, I find that paper bag use is negatively correlated with purchasing floral items and positively correlated with bakery and deli, meat and seafood, and shelf-stable food items. The type of register is not significantly correlated with paper bag use. The more trips a customer makes to the store per month (during the 1:00-4:00pm shift), the less likely they are to use paper.

At the store level, the customers in stores with the highest median incomes (>$72K) are the most likely to pay for paper bags, followed by customers at stores with middle median incomes ($55-72K). Customers at store with the lowest median incomes (< $55K) are the least likely to purchase paper. Customers at stores in areas with larger Asian populations are the less likely to choose paper bags. Finally, paper bag use is negatively correlated with the size of the store.

A.3 In-store Data Description

The in-store data were obtained through direct observation of transactions by enumerators stationed near checkout lanes. For each transaction, we collected data on the number and types of bags used, whether a bagger was present, the length of the transaction in minutes, and basic demographic data such as gender and race of the person paying. This type of transaction specific information can only be gained from in-store observations, and is not included in the scanner datasets from these stores. Four visits per store occurred in December 2013, before the Richmond DCB policy went into effect, and 4–6 visits occurred in January and February 2014, after the policy was in place. We also made an additional four visits in

March and April 2014 to collect follow-up data. Each visit lasted 1 to 2 hours and was made on either Saturday or Sunday between 11:00am and 7:00pm.

We visited a total of seven stores, belonging to two different categories of grocery chains within the same treated and control cities. The first chain is the same supermarket chain as in the main analysis of this paper, for which I also have scanner data. It is a large national chain, offering high and low prices in many products. The other chain is a regional discount chain, offering name-brand products at closeout prices.

Appendix Table A.5 presents the pre-policy summary statistics from 2013, for the in-store data collected at the national chain supermarkets (columns 1–3) and at the discount chain supermarkets (columns 4–6). Transaction level averages are presented separately for stores with and without DCB policies. Since transactions in 2013 occur before the policy change, I group the treated and no-policy stores together in the *No DCB Policy* columns and the prior-policy stores in *DCB Policy* columns. The variables recorded at checkout include indicators for the presence of baggers, a transaction being interrupted,[22] and the gender and race of the customer paying. In addition, variables for the number and types of bags used were recorded.

For the national chain stores in columns 1–3, I find that the presence of baggers, checkout interruptions, and the gender of the person paying does not differ significantly between stores with and without policies. However, the stores without DCB policies have a higher share of Black customers than the stores with DCB policies. With respect to the number and types of bags used, bag usage differs greatly between stores with and without DCB policies. In particular, the share of customers using no bags, using paper bags, and bringing reusable bags is significantly higher in stores with DCB policies in place, while the share using thin plastic is zero. Comparing discount chain averages (columns 4–6) to the national chain averages (columns 1–3), I find that discount chain transactions are less likely to have baggers present and have a shorter transaction duration than those at the national chain. Discount chain customers are also less likely to be White and are more likely to use no bags when a DCB policy is in effect.

For a more detailed discussion of the in-store data and the effects of DCB policies on bag usage at checkout, please see Taylor and Villas-Boas (2016b).

A.4 Difference-in-Differences Estimation Using In-Store Data

In Section 2.6, I matched the transactions collected in-store at the national chain to their corresponding transactions in the scanner data. With these matched data, as well as the full scanner and in-store datasets for the same stores and days, I estimate the following

[22] Interruptions include price checks, cashiers switching registers, phone calls, and the customer having payment issues such as card denial or paying by check

difference-in-differences (DID) model:

$$Y_{tsjd} = \beta_D D_{jd} + \beta_x X_{tsjd} + \theta_{sj} + \delta_d + \epsilon_{tsjd} \tag{5}$$

where Y_{tsjd} is the logged transaction duration of transaction t in store s, jurisdiction j, on day-of-sample d, D_{jd} is an indicator for transactions at the treated store during the DCB policy effective period, X_{tsjd} is a set of control variables, θ_{sj} are store fixed effects and δ_d are day-of-sample fixed effects. Appendix Table A.6 reports the results. In column (1), I estimate Equation 5 with the scanner data, controlling for the number of items purchased and amount spent. I find that $\hat{\beta}_D = 0.104$, which means that DCB policies correspond with a 10.4% increase in transaction duration at the treated store relative to the no-policy and prior-policy stores. In column (3), I estimate the model with the in-store data, controlling for the presence of baggers, checkout interruptions, the gender and race of the person paying, and the number and types of bags used. I find that $\hat{\beta}_D = 0.155$. In column (2), I estimate the model with the matched data, controlling for the covariates from both the scanner and in-store data. Once again I estimate that DCB policies lead to a positive slowdown in transaction duration ($\hat{\beta}_D = 0.099$), however, the $\hat{\beta}_D$ estimate is no longer statistically significant when using the smaller matched dataset. Yet reassuringly, comparing columns (1) and (2), the lack of in-store covariates in column (1) does not bias its $\hat{\beta}_D$ estimate.

I also find interesting patterns with respect to the control variables themselves. Controlling for all else, each additional item scanned increases transaction duration by 1-2%, while each additional dollar spent increases transaction duration by 0.2%. The presence of baggers is associated with faster transaction duration, while experiencing a checkout interruption or payment issue is associated with much slower transaction durations. Men checkout 8-11% faster than women, while the race of the person paying is not correlated with differences in checkout speed.

I also estimate Equation 5 using the in-store data from the discount chain (column 4). Comparing in-store DID results in columns (3) and (4), both chains experience similar slowdowns due to the DCB policy. Transactions at the national chain are 15.5% slower due to the policy change, which equates to 0.224 minutes longer, and transactions at the discount chain are 33.5% slower, which equates to 0.178 minutes longer. Unlike at the national chain, the presence of a bagger does not significantly alter checkout duration, yet similar to the national chain, male shoppers are faster at checkout than female shoppers, all else equal.

A.5 Appendix Tables and Figures

Table A.1: List of California DCB Policies: 2007–2014

	Jurisdiction	County	Effective Date	Population (2010)	Population w/Ban (%)
1	San Francisco	San Francisco	Oct-07	805,235	2.2%
2	Malibu	Los Angeles	Dec-08	12,645	2.2%
3	Fairfax	Marin	May-09	7,441	2.2%
4	Palo Alto	Santa Clara	Sep-09	64,403	2.4%
5	Calabasas	Los Angeles	Jul-11	23,058	2.5%
6	Unincorporated Areas	Los Angeles	Jul-11	981,861	5.1%
7	Long Beach	Los Angeles	Aug-11	462,257	6.3%
8	Santa Monica	Los Angeles	Sep-11	89,736	6.6%
9	Unincorporated Areas	Marin	Jan-12	18,451	6.6%
10	San Jose	Santa Clara	Jan-12	945,942	9.2%
11	Unincorporated Areas	Santa Clara	Jan-12	97,882	9.4%
12	Unincorporated Areas	Santa Cruz	Mar-12	130,666	9.8%
13	Manhattan Beach	Los Angeles	Apr-12	35,135	9.9%
14	Monterey	Monterey	Jun-12	27,810	9.9%
15	Sunnyvale	Santa Clara	Jun-12	140,081	10.3%
16	Pasadena	Los Angeles	Jul-12	137,122	10.7%
17	Ojai	Ventura	Jul-12	7,461	10.7%
18	Carpinteria	Santa Barbara	Jul-12	13,040	10.7%
19	Solana Beach	San Diego	Aug-12	12,867	10.8%
20	Millbrae	San Mateo	Sep-12	21,532	10.8%
21	Watsonville	Santa Cruz	Sep-12	51,199	11.0%
22	Arroyo Grande	San Luis Obispo	Oct-12	17,252	11.0%
23	Atascadero	San Luis Obispo	Oct-12	28,310	11.1%
24	Grover Beach	San Luis Obispo	Oct-12	13,156	11.1%
25	Morro Bay	San Luis Obispo	Oct-12	10,234	11.2%
26	Paso Robles	San Luis Obispo	Oct-12	29,793	11.2%
27	Pismo Beach	San Luis Obispo	Oct-12	7,655	11.3%
28	San Luis Obispo	San Luis Obispo	Oct-12	45,119	11.4%
29	Unincorporated areas	San Luis Obispo	Oct-12	118,486	11.7%
30	Fort Bragg	Mendocino	Dec-12	7,273	11.7%
31	Alameda	Alameda	Jan-13	73,812	11.9%
32	Albany	Alameda	Jan-13	18,539	12.0%
33	Berkeley	Alameda	Jan-13	112,580	12.3%
34	Dublin	Alameda	Jan-13	46,036	12.4%
35	Emeryville	Alameda	Jan-13	10,080	12.4%
36	Fremont	Alameda	Jan-13	214,089	13.0%
37	Hayward	Alameda	Jan-13	144,186	13.4%
38	Livermore	Alameda	Jan-13	80,968	13.6%
39	Newark	Alameda	Jan-13	42,471	13.7%
40	Oakland	Alameda	Jan-13	390,724	14.8%
41	Piedmont	Alameda	Jan-13	10,667	14.8%
42	Pleasanton	Alameda	Jan-13	70,285	15.0%
43	San Leandro	Alameda	Jan-13	84,950	15.2%
44	Unincorporated Areas	Alameda	Jan-13	141,368	15.6%
45	Union City	Alameda	Jan-13	69,516	15.8%

	Jurisdiction	County	Effective Date	Population (2010)	Population w/Ban (%)
46	Ukiah	Mendocino	Jan-13	16,075	15.8%
47	Unincorporated Areas	Mendocino	Jan-13	59,081	16.0%
48	Laguna Beach	Orange	Jan-13	22,723	16.0%
49	Carmel	Monterey	Feb-13	3,722	16.0%
50	West Hollywood	Los Angeles	Feb-13	34,399	16.1%
51	Dana Point	Orange	Apr-13	33,351	16.2%
52	Belmont	San Mateo	Apr-13	25,835	16.3%
53	Brisbane	San Mateo	Apr-13	4,282	16.3%
54	Burlingame	San Mateo	Apr-13	28,806	16.4%
55	Colma	San Mateo	Apr-13	1,792	16.4%
56	Daly City	San Mateo	Apr-13	101,123	16.7%
57	Half Moon Bay	San Mateo	Apr-13	11,324	16.7%
58	Menlo Park	San Mateo	Apr-13	32,026	16.8%
59	Pacifica	San Mateo	Apr-13	37,234	16.9%
60	Portola Valley	San Mateo	Apr-13	4,353	16.9%
61	San Bruno	San Mateo	Apr-13	41,114	17.0%
62	South San Francisco	San Mateo	Apr-13	63,632	17.2%
63	Unincorporated Areas	San Mateo	Apr-13	88,362	17.4%
64	Capitola	Santa Cruz	Apr-13	9,918	17.4%
65	Santa Cruz	Santa Cruz	Apr-13	59,946	17.6%
66	Mountain View	Santa Clara	Apr-13	74,066	17.8%
67	San Mateo	San Mateo	Jun-13	97,207	18.0%
68	Glendale	Los Angeles	Jul-13	191,719	18.6%
69	San Carlos	San Mateo	Jul-13	28,406	18.6%
70	Los Altos	Santa Clara	Jul-13	28,976	18.7%
71	East Palo Alto	San Mateo	Oct-13	28,155	18.8%
72	Redwood City	San Mateo	Oct-13	76,815	19.0%
73	Cupertino	Santa Clara	Oct-13	58,302	19.2%
74	Huntington Beach	Orange	Nov-13	189,992	19.7%
75	Mill Valley	Marin	Nov-13	13,903	19.7%
76	Culver City	Los Angeles	Dec-13	38,883	19.8%
77	El Cerrito	Contra Costa	Jan-14	23,549	19.9%
78	Pittsburg	Contra Costa	Jan-14	63,264	20.0%
79	Richmond	Contra Costa	Jan-14	103,701	20.3%
80	San Pablo	Contra Costa	Jan-14	29,139	20.4%
81	Los Angeles	Los Angeles	Jan-14	3,792,621	30.6%
82	South Lake Tahoe	El Dorado	Jan-14	21,403	30.6%
83	Campbell	Santa Clara	Jan-14	39,349	30.7%
84	Arcata	Humboldt	Feb-14	17,231	30.8%
85	Los Gatos	Santa Clara	Feb-14	29,413	30.9%
86	Santa Barbara City	Santa Barbara	Mar-14	88,410	31.1%
87	Morgan Hill	Santa Clara	Apr-14	37,882	31.2%
88	Truckee	Nevada	Jun-14	16,180	31.2%
89	Beverly Hills	Los Angeles	Jul-14	34,109	31.3%
90	Davis	Yolo	Jul-14	65,622	31.5%
91	Walnut Creek	Contra Costa	Sep-14	64,173	31.7%
92	Tiburon	Marin	Sep-14	8,962	31.7%
93	Desert Hot Springs	Riverside	Sep-14	25,938	31.8%
94	Cloverdale	Sonoma	Sep-14	8,618	31.8%
95	Cotati	Sonoma	Sep-14	7,265	31.8%

	Jurisdiction	County	Effective Date	Population (2010)	Population w/Ban (%)
96	Healdsburg	Sonoma	Sep-14	11,254	31.9%
97	Petaluma	Sonoma	Sep-14	57,941	32.0%
98	Rohnert Park	Sonoma	Sep-14	40,971	32.1%
99	Santa Rosa	Sonoma	Sep-14	167,815	32.6%
100	Sebastopol	Sonoma	Sep-14	7,379	32.6%
101	Sonoma	Sonoma	Sep-14	10,648	32.6%
102	Unincorporated Areas	Sonoma	Sep-14	146,006	33.0%
103	Windsor	Sonoma	Sep-14	26,801	33.1%
104	San Rafael	Marin	Sep-14	57,713	33.2%
105	South Pasadena	Los Angeles	Oct-14	25,619	33.3%
106	Novato	Marin	Oct-14	51,904	33.4%
107	Sausalito	Marin	Oct-14	7,061	33.5%
108	Larkspur	Marin	Nov-14	11,926	33.5%
109	Indio	Riverside	Nov-14	76,036	33.7%
110	Palm Springs	Riverside	Nov-14	44,552	33.8%
111	Santa Clara	Santa Clara	Dec-14	116,468	34.1%
	Statewide			37,253,965	

Source: Author's calculations. Population statistics come from U.S. Census Bureau, Census 2010.

Table A.2: Event Study Regression Output for Figure 2.3a

			$Y_{sjw} = ln(TxnDuration)$		
D_{-24}	-0.005	D_{-5}	-0.006	D_{15}	0.014
	(0.010)		(0.012)		(0.013)
D_{-23}	-0.009	D_{-4}	0.001	D_{16}	0.022
	(0.012)		(0.010)		(0.013)
D_{-22}	0.002	D_{-3}	-0.004	D_{17}	0.027**
	(0.011)		(0.011)		(0.012)
D_{-21}	-0.018	D_{-2}	0.004	D_{18}	0.028*
	(0.014)		(0.010)		(0.014)
D_{-20}	0.002	D_{0}	0.037***	D_{19}	0.012
	(0.011)		(0.008)		(0.012)
D_{-19}	-0.012	D_{1}	0.029***	D_{20}	0.028**
	(0.014)		(0.0110)		(0.014)
D_{-18}	0.003	D_{2}	0.051***	D_{21}	0.025
	(0.011)		(0.008)		(0.015)
D_{-17}	-0.0021	D_{3}	0.021	D_{22}	0.023**
	(0.012)		(0.014)		(0.012)
D_{-16}	-0.010	D_{4}	0.048***	D_{23}	0.023
	(0.013)		(0.0120)		(0.014)
D_{-15}	0.000	D_{5}	0.030***	D_{24}	0.025*
	(0.012)		(0.010)		(0.015)
D_{-14}	0.004	D_{6}	0.030**		
	(0.013)		(0.012)		
D_{-13}	-0.019*	D_{7}	0.021		
	(0.010)		(0.014)		
D_{-12}	-0.010	D_{8}	0.030**		
	(0.012)		(0.014)		
D_{-11}	-0.001	D_{9}	0.036***		
	(0.013)		(0.011)		
D_{-10}	-0.001	D_{10}	0.021		
	(0.011)		(0.014)		
D_{-9}	-0.006	D_{11}	0.013		
	(0.014)		(0.012)		
D_{-8}	-0.005	D_{12}	0.010		
	(0.011)		(0.011)		
D_{-7}	0.001	D_{13}	0.023***		
	(0.012)		(0.008)		
D_{-6}	-0.006	D_{14}	0.024**		
	(0.010)		(0.009)		
		Num of Obs.		9381	
		Standard Errors		Cluster	
		Store FE		Yes	
		Week-of-sample FE		Yes	

Note: The table presents the results from event study Equation 2.1, as plotted in Figure 2.3a. The dependent variable is logged average transaction duration, measured in minutes, in store s, jurisdiction j, and week-of-sample w. Standard errors are in parentheses, estimated using two-way error clustering at the policy jurisdiction and week-of-sample level. Asterisks indicate: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A.3: Correlates of Paper Bag Choice (*Customer-Month Averages*)

	(1) Purchasing Paper (share)		(2) Purchasing Paper (share)	
	Coeff.	Std. Err.	Coeff.	Std. Err.
Customer-Month Variables				
Q2: 4-8 Items Scanned (share)	0.126***	0.006	0.112***	0.005
Q3: 9-18 Items Scanned (share)	0.178***	0.007	0.156***	0.006
Q4: > 18 Items Scanned (share)	0.142***	0.005	0.128***	0.006
Amount/Item ($)	0.004***	0.000	0.001***	0.000
Alcohol & Tobacco (share)	0.010***	0.002	0.001	0.002
Bakery & Deli (share)	0.005***	0.002	0.011***	0.001
Dairy & Refrigerated Items (share)	-0.001	0.003	0.017***	0.001
Floral (share)	-0.015***	0.004	-0.010***	0.002
Frozen Items (share)	-0.002	0.003	0.008***	0.001
Meat & Seafood (share)	0.024***	0.002	0.022***	0.002
Fresh Produce (share)	-0.012***	0.002	0.016***	0.002
Shelf-Stable Food Items	0.022***	0.002	0.013***	0.001
Baby Items (share)	0.041***	0.005	0.003	0.004
Pet Items (share)	-0.000	0.003	-0.014***	0.002
Full-service Lane (share)	-0.016	0.010	-0.010	0.009
Express Lane (share)	0.000	0.009	0.004	0.008
Trips/Month (#)	-0.001	0.001	-0.002***	0.000
Store Variables				
Median Income $55-72K (=1)	0.026**	0.009		
Median Income > $72K (=1)	0.054***	0.014		
Household Size (#)	0.000	0.016		
White (share)	-0.266**	0.123		
Black (share)	-0.192	0.113		
Asian (share)	-0.497***	0.107		
Over 65 (share)	-0.032	0.083		
Do not own vehicle (share)	0.314	0.280		
Remodel Date (year)	-0.001	0.002		
Store Open Date (year)	-0.000	0.000		
Size (1000 ft^2)	-0.001***	0.000		
Self-checkout (=1)	0.001	0.013		
Urban (=1)	-0.006	0.008		
N	674,546		670,494	
R squared	0.037		0.376	
Standard Errors	Cluster		Cluster	
Customer FE	No		Yes	
Month-of-Sample FE	Yes		Yes	
Months-since-Policy FE	Yes		Yes	

Note: The dependent variable is the share of customer's i transactions where paper bags were purchased in month m and store s. Asterisks indicate the following: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses, estimated using two-way error clustering at the policy jurisdiction and month-of-sample level. *Source*: Customer-month variables come from author's calculations using the scanner data. Store variables were provided by the retailer, or come from Gicheva et al. (2010), who use 2000 US Census data for each store's census block-group. Urban areas are locations with populations densities greater than 500 people per square mile.

Table A.4: Effect of DCB Policies on Share of Transactions Purchasing Item Group (*Store-Week Averages*)

	(1) Produce	(2) Meat and Seafood	(3) Dairy and Refrigerated	(4) Frozen	(5) Bakery and Deli
Ban Effective Dummy	-0.004*	-0.002	-0.002	-0.000	0.001
	(0.002)	(0.001)	(0.002)	(0.001)	(0.0012)
Mean Y_{sw} (2011)	0.52	0.34	0.467	0.26	0.28
	(6) Shelf-Stable Food	(7) Alcohol and Tobacco	(8) Baby Items	(9) Floral	(10) Pet
Ban Effective Dummy	-0.000	0.000	-0.000	0.000	-0.000
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Mean Y_{sw} (2011)	0.73	0.19	0.02	0.04	0.05
Num of Obs.	9381	9381	9381	9381	9381
Standard Errors	Cluster	Cluster	Cluster	Cluster	Cluster
Store FE	Yes	Yes	Yes	Yes	Yes
Week-of-sample FE	Yes	Yes	Yes	Yes	Yes

Note: The table presents the results from difference-in-differences Equation 2.2. The dependent variable is share of transactions in store s, jurisdiction j, and week-of-sample w purchasing items in the following categories (1) produce, (2) meat and seafood, (3) dairy and refrigerated, (4) frozen, (5) bakery and deli, (6) shelf-stable food, (7) alcohol and tobacco, (8) baby, (9) floral, and (10) pet. Standard errors are in parentheses, estimated using two-way error clustering at the policy jurisdiction and week-of-sample level. Asterisks indicate: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A.5: Average Transaction Level Characteristics in 2013 (*In-store Data*)

	(1)	(2)	(3)	(4)	(5)	(6)
	National Chain			Discount Chain		
	No DCB Policy	DCB Policy	Diff.	No DCB Policy	DCB Policy	Diff.
Bagger Present (=1)	0.74	0.69	0.048	0.47	0.08	0.392***
Checkout Interruption (=1)	0.06	0.07	-0.007	0.03	0.03	-0.007
Male (=1)	0.41	0.43	-0.021	0.42	0.53	-0.115***
White (=1)	0.65	0.71	-0.059	0.39	0.45	-0.054
Black (=1)	0.14	0.09	0.055*	0.28	0.31	-0.026
No Bag (=1)	0.05	0.15	-0.101***	0.06	0.24	-0.185***
Plastic Bag (=1)	0.86	0.00	0.865***	0.90	0.00	0.897***
Paper Bag (=1)	0.05	0.38	-0.331***	0.00	0.12	-0.115***
Bought Reus. Bag (=1)	0.00	0.02	-0.019**	0.00	0.19	-0.186***
Brought Reus. Bag (=1)	0.11	0.55	-0.440***	0.07	0.51	-0.446***
Plastic Bag (#)	4.03	0.00	4.030***	3.30	0.00	3.301***
Paper Bag (#)	0.16	0.75	-0.589***	0.00	0.28	-0.282***
Bought Reus. Bag (#)	0.00	0.03	-0.032**	0.00	0.31	-0.308***
Brought Reus. Bag (#)	0.26	1.22	-0.968***	0.09	0.88	-0.793***
Transaction Duration (min.)	1.49	1.72	-0.236**	1.31	1.59	-0.278***
N Obs.	333	157		631	156	

Note: Since 2013 is the pre-policy period for the treated stores, I group the treated and no-policy stores together (*No DCB policy*) and the prior-policy stores (*DCB policy*). Asterisks indicate the following: * p < 0.10, ** p <0.05, *** p <0.01. *Source*: Author's calculations from observational data collected in-store at checkout.

Table A.6: Effect of DCB Policies on Transaction Duration (*Scanner vs. In-store Data*)

	(1)	(2)	(3)	(4)
		National Chain		Discount Chain
	Scanner Data	Matched Data	In-store Data	In-store Data
DCB Effective (=1)	0.104***	0.099	0.155**	0.335***
	(0.026)	(0.0907)	(0.062)	(0.050)
Items Scanned (#)	0.024***	0.009***		
	(0.001)	(0.002)		
Amount Spent ($)	0.003***	0.002***		
	(0.002)	(0.001)		
Bagger Present (=1)		-0.062**	-0.092***	0.003
		(0.028)	(0.024)	(0.034)
Interruption (=1)		0.561***	0.576***	0.586***
		(0.079)	(0.066)	(0.052)
Male (=1)		-0.083***	-0.107***	-0.139***
		(0.021)	(0.021)	(0.019)
White (=1)		-0.060	-0.019	0.041
		(0.053)	(0.024)	(0.024)
Black (=1)		0.054	0.034	0.000
		(0.060)	(0.038)	(0.035)
No Bags (=1)		-0.197***	-0.055	-0.157***
		(0.056)	(0.048)	(0.034)
Plastic Bag (#)		0.035***	0.083***	0.105***
		(0.005)	(0.005)	(0.005)
Paper Bag (#)		0.088***	0.219***	0.237***
		(0.015)	(0.012)	(0.022)
Bought Reusable (#)		0.183***	0.354***	0.315***
		(0.059)	(0.047)	(0.021)
Brought Reusable (#)		0.096***	0.226***	0.243***
		(0.018)	(0.009)	(0.011)
Num of Obs.	22,061	687	1,692	2,228
Standard Errors	Cluster	Cluster	Cluster	Cluster
Store FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
Register FE	Yes	Yes	Yes	Yes
Cashier FE	Yes	No	No	No
Hour FE	Yes	No	No	No
Mean Y_{tsd}	1.838	1.766	1.718	1.446

Note: The table presents the results from difference-in-differences Equation 5. The dependent variable is logged average transaction duration, measured in minutes, for transaction t in store s and date d. Standard errors are estimated using error clustering at the store-day level. Asterisks indicate the following: $*p < 0.10$, $**\,p < 0.05$, $***p < 0.01$. For the national chain, column (1) presents the model with the full sample of scanner data, column (3) presents the model with the full sample of in-store observational data, and column (2) presents the model with only the transactions matched between the scanner and in-store data. For the discount chain, column (4) presents the model with the full sample of in-store data.

Table A.7: Average Store and Transaction Characteristics from Pre-Policy Period (*DC Data*)

	Control	Treat	Diff.
Building Characteristics			
Building Size (ft^2)	42,990.17	45,927.67	-2937.500
Open Year	1985	1981	4.000
Register Characteristics			
Self-checkout (share)	0.17	0.33	-0.167
Full-service Registers (#)	4.83	6.00	-1.167
Express Registers (#)	4.08	3.67	0.417
Demographic Characteristics			
Median Income ($)	$67,379.92	$58,345.83	9034.083
Household Size (#)	2.41	2.20	0.203
White (share)	0.58	0.40	0.178
Black (share)	0.26	0.53	-0.275
Asian (share)	0.08	0.02	0.057**
Over 65 (share)	0.11	0.15	-0.046**
Do not own vehicle (share)	0.11	0.27	-0.166***
Urban (share)	1.00	1.00	0.000
Transaction Length (minutes)			
Full Sample	1.80	2.02	-0.224**
Smallest Txns. (scans < 8)	1.23	1.29	-0.062
Largest Txns. (scans ≥ 8)	2.42	2.73	-0.314**
Items Scanned (#)			
Full Sample	10.98	12.08	-1.093
Smallest Txns. (scans < 8)	3.72	3.70	0.020
Largest Txns. (scans ≥ 8)	18.98	20.18	-1.201
Amount Paid ($)			
Full Sample	32.77	35.04	-2.274
Smallest Txns. (scans < 8)	12.67	11.88	0.791*
Largest Txns. (scans ≥ 8)	54.65	57.33	-2.678
N Stores	12	6	–

Note: Asterisks indicate the following: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Store characteristic data were provided by the retailer. Store demographic data come from Gicheva et al. (2010), who use 2000 US Census data for each store's census block-group. Transaction characteristics obtained from author's calculations using the scanner data.

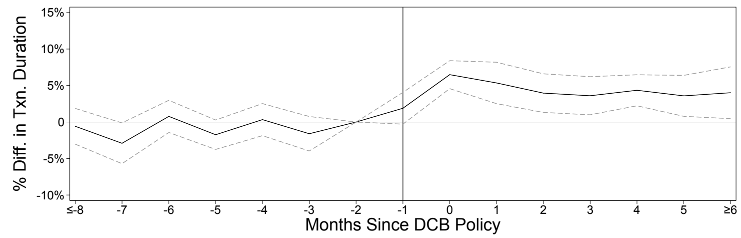
Table A.8: Store Heterogeneity: Effect of DCB Policies on Transactions per Shift (*Store-Week Averages*)

	Log Transactions per Shift (#)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ban Effective	-0.030***	-0.028***	-0.011	-0.030**	-0.025**	-0.014	-0.041***	-0.013
	(0.010)	(0.009)	(0.011)	(0.012)	(0.010)	(0.014)	(0.011)	(0.018)
Ban Effective × Large Building Size		-0.004						-0.006
		(0.015)						(0.015)
Ban Effective × Med. Inc. \$55–72K			-0.023					-0.036*
			(0.018)					(0.019)
Ban Effective × Med. Inc. > \$72K			-0.043***					-0.066***
			(0.012)					(0.023)
Ban Effective × High Asian Share				0.000				0.045**
				(0.015)				(0.019)
Ban Effective × High Black Share					-0.010			-0.046***
					(0.017)			(0.015)
Ban Effective × Urban						-0.020		0.011
						(0.017)		(0.020)
Ban Effective × Low Vehicle Share							0.022	0.011
							(0.014)	(0.019)
Num of Obs.	9381	9381	9381	9381	9381	9381	9381	9381
R squared	0.966	0.966	0.966	0.966	0.966	0.966	0.966	0.967
Standard Errors	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Covariates X_{sw}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Store FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week-of-Sample FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

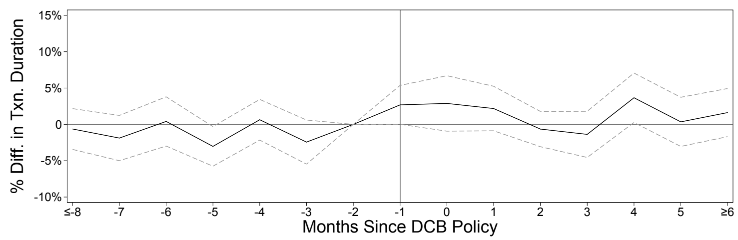
Note: The table presents the results from Equation 2.2. The outcome variable is the logged average number of transactions completed per 3 hour shift in store s, jurisdiction j, and week w. Column (1) replicates the log specification from column (2) of Table 2.4. Columns (2) through (7) include interactions of the ban effective dummy and dummies for store s being (i) above median with respect to building size, (ii) in a census block group with median income either $< \$55K$ (*omitted*), \$55–77K, or $> \$72K$, (iii) in a census block group with an above median share of Asian residents, (iv) in a census block group with an above median share of Black residents, (v) in an urban census block group, and (vi) in a census block group with lower than median vehicle ownership. Column (8) includes all interactions with the ban effective dummy. Standard errors are estimated using two-way error clustering at the policy jurisdiction and week-of-sample level. Asterisks indicate the following: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Figure A.1: Effect of DCB Policies on Transaction Duration (*Customer-Month Averages*)

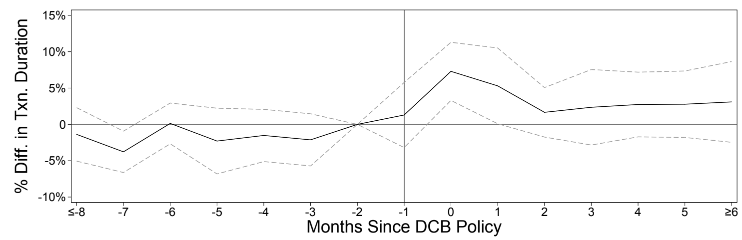
(a) All Households



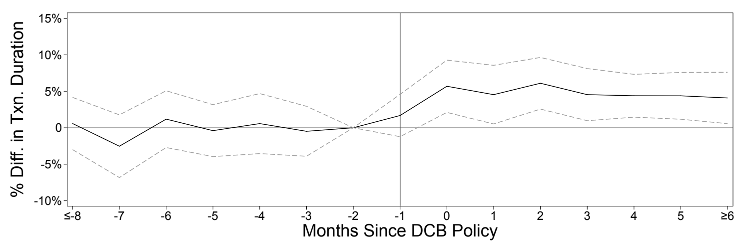
(b) Never Purchased Paper, Small Txn. Size. (Scans < 8)



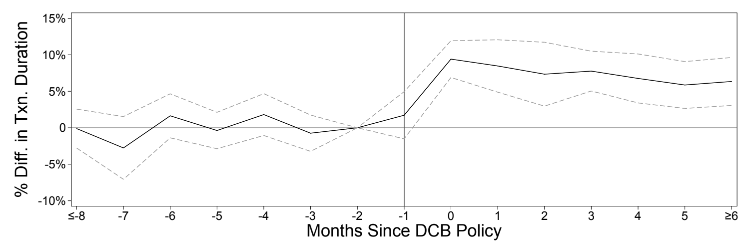
(c) Purchased Paper, Small Txn. Size. (Scans < 8)



(d) Never Purchased Paper, Large Txn. Size. (Scans ≥ 8)



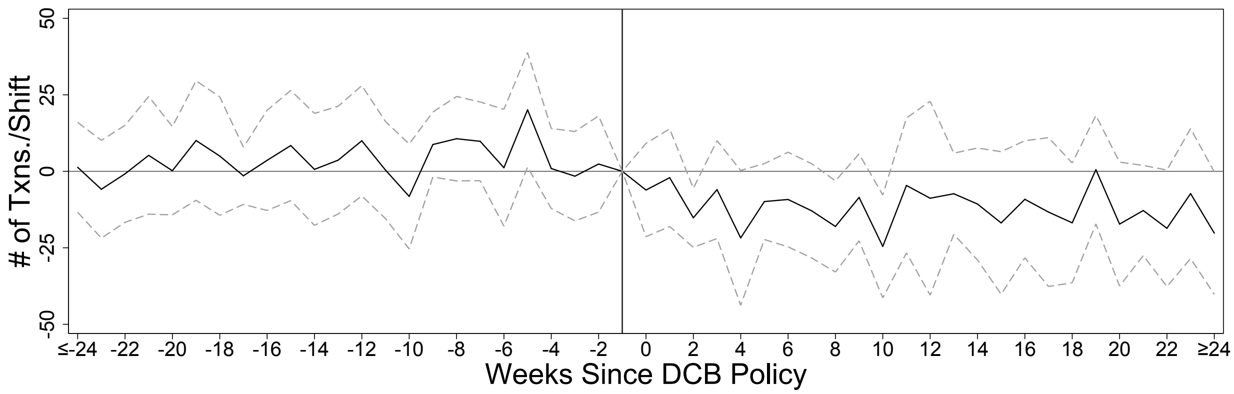
(e) Purchased Paper, Large Txn. Size. (Scans ≥ 8)



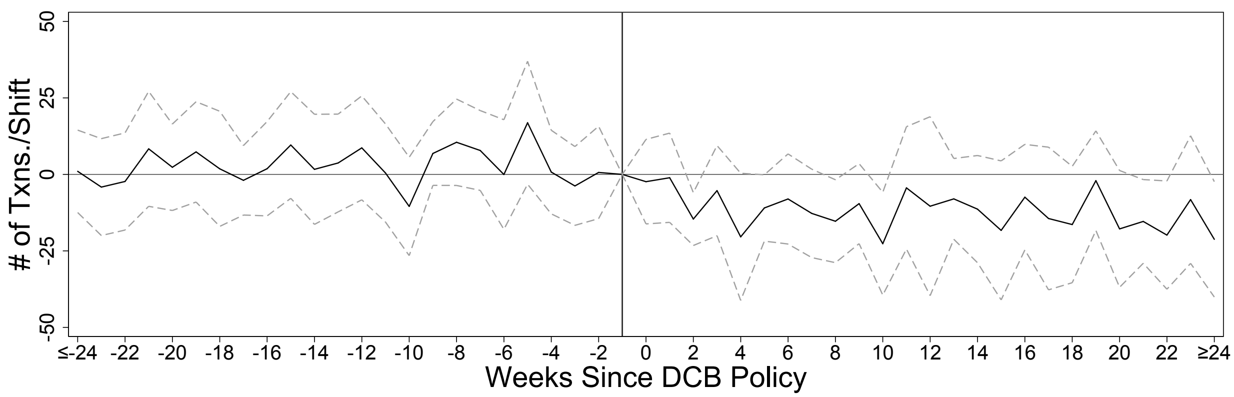
Note: Figure presents the full specification of event study Equation 3, with customer and month-of-sample fixed effects and control variables for the average number of items purchased per transaction, the amount spent per transaction, the types of items purchased, and months in sample—by customer i in store s, jurisdiction j, and month-of-sample m. The dependent variable is logged average transaction duration, measured in minutes, for customer i in store s, jurisdiction j, and month-of-sample m, for the entire sample of customers (panel a) and for all control customers compared to subsets of treated customers by transaction size and paper bag use (panels b–e). Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample.

Figure A.2: Effect of DCB Policies on Number of Transactions Completed per Shift (*Store-Week Averages*)

(a) Transactions per Shift—Without Control Variables



(b) Transactions per Shift—With Control Variables



Note: The figure panels display the $\hat{\beta}_l$ coefficient estimates from event study Equation 2.1. The dependent variable is the number of transaction processed per 1:00-4:00pm shift in store s, jurisdiction j, and week-of-sample w. Upper and lower 95% confidence intervals are depicted in gray, estimated using two-way cluster robust standard errors on policy jurisdiction and week-of-sample. Panel (a) presents the specification of Equation 2.1 with event study indicators, store fixed effects, and week-of-sample fixed effects. The specification in panel (b) additionally includes control variables, X_{sjw}, for average transaction size, average transaction expenditures, and the share of transactions purchasing each of the following items—produce, meat/seafood, dairy/refrigerated, frozen, bakery/deli, shelf-stable food, alcohol/tobacco, infant/toddler, floral department, and pet items—in store s, jurisdiction j, and week-of-sample w.